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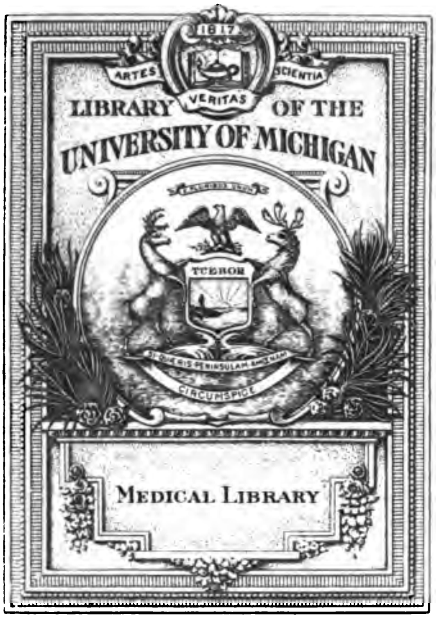

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THE

NEW-YORK

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JOURNAL

AND

REVIEW.

VOL. I.

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MEDICAL AND PHILOSOPHICAL Journal and Review.

Vol. I.—No. 1.

To the Editors of the Medical and Philosophical Journal and Review.

Gentlemen,

The following Theory of the Proximate Cause of Fever was delivered by the Author while Professor of the Institutes of Medicine. It will be seen from the aphoristic form in which it is written, that it was originally intended rather as a text book for his lectures, than as a finished treatise on the subject. If, in this imperfect state, it is thought worthy of being published, you are at liberty to insert it in your Journal.

Outlines *of a* new Theory *of the* Proximate Cause *of* Fever: *By* Benjamin De Witt, *M. D. Professor of Chemistry in the University of the State of New-York, and late Professor of the Institutes of Medicine.*

1. As the circulation of the blood is necessary for the preservation of life and health; so a perfect knowledge of that function is necessary to enable the Physician to prevent disease and death. Before the discovery of the circulation, doubts and difficulties attended the science of Medicine. Diseases in general, and those of a febrile nature in particular, were imperfectly understood. Indeed, it was impossible for any one to comprehend their true cause, or to point out the proper re-

medies for their cure. But after that important discovery, it could not escape the most superficial observer, that fever invariably produced more or less disturbance in the sanguiferous system. Accordingly it has been the universal practice of Physicians, from Dr. Harvey down to those of the present day, to resort to the state of the pulse as a characteristic indication of the existence of the disorder. There are indeed many considerations which lead to the conclusion, that the blood-vessels are the principal seat of fever; and this opinion is now so generally received, that I presume it will not be controverted.

2. To understand, therefore, the nature of fever, it is necessary to attend minutely to the physiology of the sanguiferous system, and to examine particularly the action of the heart and arteries.

3. The heart is a hollow muscle, whose action consists in alternate contractions and dilatations. The blood, after being received into the ventricles during the dilatation of the heart, is prevented by the valves from returning into the veins. The natural effect, therefore, of the contractions of the heart is to propel the blood into the arteries more or less forcibly, more or less frequently, or with greater or less regularity.

4. We distinguish accordingly three varieties of action in the heart different from each other. The first comprehends all the possible degrees of force in the contractions, from the strongest to the weakest action. The second includes every degree of celerity, from the slowest to the most frequent pulsations; and the last embraces every species of irregularity in motion.

5. The impulse given to the blood by each contraction of the heart, produces a mechanical dilatation of the elastic arteries into which it is propelled. These dilatations may be accurately felt by pressing the fingers on any one of the large arteries. Hence results the doctrine of the pulse so far as it relates exclusively to the action of the heart.

6. If, indeed, the arteries were mere elastic tubes, without any power of action in themselves, the pulse would be simple and

easily understood. It would indicate the motions of the heart only, and mark the few varieties of action of which it is capable.

7. But the mechanical dilatation of the arteries produced by the impulse of the blood, is variously modified and changed by the contractions of the arteries themselves. And this muscular action of the arteries in its turn again exerts an influence upon the action of the heart. The pulse therefore not only indicates the several varieties and degrees of action in the heart, but also the different kinds and degrees of action in the arterial system. And this combination of movements in the heart and arteries, acting and re-acting at the same time on each other, produces the endless variety of complicated motions distinguishable in the pulse.

8. These motions of the sanguiferous system, that is, of the arteries as well as of the heart, though complex in their nature, and difficult to be understood, are, nevertheless, of great importance, and should be accurately attended to in the science and practice of Medicine; especially in fever, which, being seated chiefly in the arterial system, immediately and greatly affects the action of the heart and arteries, and produces corresponding changes in the pulse.—Let us, therefore, examine more particularly the action of the arterial system.

9. Mr. John Hunter has remarked, that the elastic is almost the only power with which the coats of the large arteries are invested: while irritability is very apparent in those of a smaller diameter, and almost exclusively predominates in the capillary vessels. It is presumed accordingly, that in proceeding from the heart, the elastic power diminishes with the diminution of the size of the arteries, until it is lost in their extremities; and that the muscular power is increased in the same proportion, until it prevails exclusively in the termination of the capillaries: so that in the small arteries, at a medium between the two extremities, the two powers of elasticity and muscularity are equally balanced, and both operate equally on the circulation of the blood.

10. With a view, then, of speaking with greater precision on this subject, I shall divide the arteries into three systems of vessels: The system of large arteries; the system of small arteries; and the system of capillaries. And although the exact limits between these systems cannot be defined with anatomical precision, yet, in a physiological point of view, while we consider that they are capable of different kinds of action, subject to different influences, and liable to distinct habits of motion, this division seems to be appropriate and necessary.

11. Having premised thus much concerning the moving powers of the arterial system, I proceed to consider more particularly the muscular action of the arteries: and here I shall remark generally, that they are capable of three distinct kinds of actions, essentially different in their nature from each other. Their actions may be either *natural*, *reversed*, or *convulsed*. The first occurs always in a state of health; the second takes place often in disease; and the last seldom happens except at the approach of death. I shall illustrate each of them in the order they are mentioned.

12. The *natural action* of the arteries consists principally in the successive contractions of their circular fibres in a progressive order and direction from the heart to their extremities: thus accelerating the motion of the blood, and co-operating with the action of the heart in promoting the circulation.

13. These successive contractions of the muscular fibres of the arteries being induced by the momentum of the blood from every pulsation of the heart, correspond for the most part with the action of that organ; that is, the sudden distention given to these muscular fibres, by the impulse of every wave of blood propelled from the heart, causes each one of them successively to contract, as soon as the wave reaches it; so that the velocity of these trains of contractions keeps pace with the velocity of the impulse given to the blood.

14. This is more especially the case in the large arteries; on them the impulse of the blood is always great in proportion to

the force of contraction in the heart, and their elastic predominates over their muscular power; so that the action of the moving fibres is subordinate to the contractile force of elasticity. The muscular fibres, therefore, do assist the contractions of these arteries, but can never control them. Hence the trains of contractions in these arteries are under the exclusive dominion of the heart, and are habitually regulated by its action.

15. The small arteries, as they are farther removed from the influence of the heart and the impulse of the blood than the large ones; and as they are endowed with a proportionally greater degree of muscular power; so they are capable of exerting a more independent and energetic action: and although the force and velocity of the trains of contractions in them may usually be governed by the impulse of the blood; yet it is evident that their action is occasionally increased or diminished in consequence of their connection with the capillaries, and from the influence of external causes. In confirmation of this fact it is to be observed, that as these small arteries occupy a middle position between the large and capillary vessels, and partake of the predominant moving powers of both; so they must necessarily be governed more or less by the causes which actuate both, in proportion as they approach nearer to the one or to the other of them.

16. The capillary arteries are so minute in their capacity, and so distant from the centre of action in the heart, that the impulse of the blood is scarcely perceptible in them, and so feeble that it is insufficient to govern their trains of contractions: and hence, particularly in those capillaries which are spread on the surface of the body, we observe a continual change of motion without any corresponding change in the heart or large arteries. The action of these capillary vessels is influenced by every vicissitude of heat and cold, and they may be excited with great facility into preternatural activity by the slightest causes, without affecting the action of the heart. Hence, by the par-

tial application of heat to the skin, or the mere stimulus of friction, redness and inflammation may be induced.

17. The *reversed action of the arteries*, like the reversed action of all other muscular tubes in the body, consists in the successive contractions of their circular fibres, in a manner similar in every respect to the natural action of those fibres, except in their order of motion, which is changed, and consequently proceeds in a direction opposite to their natural one. This reversed action of the arteries, however, cannot take place throughout the whole of the arterial system, but must necessarily be confined to the capillaries and small arteries; as the large branches, from their structure and organization, admit of no other than the natural action, which will be explained more fully hereafter.

18. The reversed action of the capillaries and small arteries, although it operates in a direction opposite to the course of the blood, yet it does not prevent, but only retards, the motion of that fluid in those vessels. As in the natural action their cavities are not obliterated, so neither are they in the reversed action, but admit the constant passage of the blood, which continues to move in its accustomed course, though with some difficulty, by means of the action of the heart and large arteries. The reversed action is, moreover, generally weaker than the natural action of the same vessels under similar circumstances would be; and this contributes to lessen the resistance to the motion of the blood, which otherwise it might be supposed to produce. This resistance, however, has always the effect of accumulating the blood in greater or less quantity in the large arteries; and this again increases the action of the heart, which operates additionally to counterbalance the obstruction given to the circulation by the reversed action.

19. It is to be observed further, that the reversed action of the arteries is always emphatically and intrinsically a morbid action, in as much as it is directly opposed to the natural or healthy action, and always counteracts in a greater or less degree

the free circulation of the blood. Although it may be partial in relation to the whole of the arterial system, yet, whenever it takes place in any considerable portion of the capillaries at the same time, and more especially when it extends to the system of small arteries, it always produces a disturbance in the action of the heart and large arteries, and greatly affects the general circulation of the blood and all the functions of the body connected with it.

20. The reversed action of the arteries is subject to every possible degree of variation in force and extent. As the capillaries of the skin may be partially excited into preternatural activity by external causes, without an affection of the small or large arteries; so likewise the action of the capillaries may be reversed without extending to the other systems of vessels: but whenever this reversed action becomes more general in the system of capillaries, from the operation of general causes, or from the effects of association of motion, it extends its influence to the system of small arteries, and indirectly affects the action of the heart; in like manner as the more general excitement of the capillaries always increases the general circulation.

21. If the capillary vessels were like the large arteries, endowed with a strong elastic power, and with comparatively few muscular fibres; and if the pulsations of the heart were as strong in them as in the large arteries; it is evident that no reversed action of the arterial system could happen, so lông as the heart continued to beat. The organization of the arteries, and the action of the heart would then most effectually prevent it.

22. If, on the contrary, the large arteries were, like the capillaries, exclusively endowed with muscular power, and their trains of contractions not excited by the action of the heart on the blood, but under the control of other causes, and especially of stimulants operating from without, it is equally evident that the whole of the arterial system would be capable of reversed

action, from the same causes which induce this kind of morbid action in the other muscular tubes of the body.

23. But neither of these conditions exists in the whole of the arterial system. On the contrary, each of them prevails in the opposite extremities of that system, and they run into each other in an opposite direction. It follows, therefore, that one portion of the arteries, to wit, that which is nearest the extremities, is, from its very structure and habitude of motion, capable of reversed action; and that the other part, to wit, that which is nearest the heart, for the same reasons, is not. Hence it is to be observed, that the reversed action is always partial and never general in the arterial system. While the natural trains of contractions which commence at or near the heart, continue in the course of the large arteries towards their extremities; the reversed trains of contractions commence at or near the extremities of the capillaries, and follow each other successively up the course of the small arteries, until their power is overcome by the superior energy of the natural trains of action proceeding from the heart; or until they are lost in the predominating power of elasticity in the large arteries. For this reason, the reversed action cannot extend farther up the course of the vessels, than to the point where the pulsations of the heart control the trains of contractions, and where the elastic power begins to prevail over the muscular force of the arteries: and this point will recede farther from, or approach nearer to the heart, in proportion as the power of action in that organ is occasionally increased or diminished, and in proportion to the force and violence of the causes producing reversed action.

24. From the experiments of Baron Haller, the father of Physiology, it appears that in microscopic observations on the circulation, it is usual to see globules of blood regurgitating from the capillary arteries again and again before they pass through them. This can happen only in consequence of their reversed action. After speaking of the natural course of the blood in

the arteries, he adds, "Yet instances occur when from the "passions of the mind, from a sudden and great revulsion by "blood-letting, or from convulsion, the blood has retroceded "from the small into the large arteries. And in like manner, "from an obstruction being formed in the venous branches "above the valves, the blood has been known to return into the "extreme branches." In another place he remarks, that "the "anastamoses of the arteries remove every danger of obstruc-"tion, for in any part of an artery where there may be an in-"cipient obstruction or coagulation of the blood, they admit an "opposite current, by which the obstruction is repelled in a con-"trary direction into a larger part of a trunk." It is therefore demonstrated, beyond the possibility of a doubt, that the capillary arteries are capable of a reversed action to a certain extent.

25. This reversed action of the arteries, in consequence of obstruction, is conformable to what happens in all the other vessels or muscular tubes in the body. And there is such a perfect analogy between all the vascular parts of the system, that the fact of reversed action in the arteries, even if it had not been proved by actual observation, might have been inferred from their structure, and rendered sufficiently certain by analogical reasoning, from the action of other vessels of similar organization.

26. Thus in the venous system, according to the observation of Haller already quoted, in consequence of an obstruction formed in the large veins, the blood has been returned, by a reversed action of the vessels, into the extreme branches. Thus in the alimentary canal, in consequence of the obstruction produced by strangulated hernia, the intestines are excited into reversed action, and the most violent vomiting is induced, attended with the discharge of fæcal matter by the mouth. Thus in the lymphatic system, by the reversed action of a part of that system, the urine, after being absorbed from the bladder, has been returned into the stomach, and vomited up for months

together, as in the case related by Dr. Senter in the Transactions of the College of Physicians of Philadelphia.* Thus also in the gall ducts, the ureters, and the urethra, in consequence of the obstruction of calculi descending into them, they are excited into reversed action, and return the offending matter into the several receptacles in which they were formed.†

27. In short, the susceptibility of reversed action in muscular tubes appears to be general, and perhaps without exception. The similitude of action therefore in the organs of this struc-

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* In this case Dr. Senter cites a number of other cases to prove the reversed action of the lymphatic and absorbent systems.

† With respect to the reversed action of the lymphatics, it may be objected that the valves effectually prevent the retrograde motion of the fluids, and that this is proved by the impracticability of injecting these vessels in a dead subject in a direction contrary to the natural motion of the fluids. While I admit, this fact, I by no means admit the conclusion which is drawn from it. There is a vast difference between a dead and a living subject, between the motions of dead and living matter. The valves in their dead state may effectually close the vessels, but in their living state their muscular fibres may, by the pressure of preternatural fluids and preternatural stimulants, be excited into contractions, which may at least partially open the vessels, so as to admit the retrograde motion of the fluids. This is highly probable in itself, and it is confirmed by many facts, which can be explained in no other way than by admitting a reversed action of the lymphatics. But whatever may be the fate of the doctrine of reversed motion in the fluids of the lymphatic system, it cannot effect my theory. The very existence of the valves in that system, which no one will deny, is to me demonstrative evidence of the truth of my position of the capability of reversed action. For what is the use and design of these valves? Evidently to prevent, in a certain degree, the effect of retrograde action. And why is a provision made to prevent a thing which cannot happen—to counteract a power which cannot exist? The presence of the valves therefore conclusively shows the susceptibility of reversed action; for if this could not take place, there would be no use for valves.

In the arterial system no valves are necessary to prevent the retrograde motion of the fluid they contain. The vis-a-tergo of the heart and arteries effectually prevents the reversed motion of the blood in the large vessels which do not admit of reversed action in consequence of their peculiar organization. But this power can by no means prevent the reversed action of the capillaries and small arteries, as will be presently shown.

ture is to be considered as universal, and the analogy between them complete. To suppose, then, that the capillary arteries alone are incapable of this kind of motion, when from their structure they evidently admit of it, and when we know that the other vessels of like conformation are liable to it, would be contrary to the strictest analogical reasoning, and totally inadmissible in physiological research.

28. The facility and frequency of the recurrence of reversed action in the vascular organs of the body is another circumstance that requires attention. And here also the analogy subsisting between them is conspicuous. Thus in ruminating animals the motion of the gullet in either direction is under the control of the voluntary power—and the muscular fibres are excited into successive contractions in an order of motion from the stomach to the mouth, as readily as from the mouth to the stomach. Hence the aliment is regurgitated as easily as it is swallowed; and this faculty of voluntary reversed action of the œsophagus is said also to have been acquired in some cases by the human subject. Thus also the act of vomiting is induced with great facility by the slightest causes; for in certain states of the body, a morsel of unsavoury food, an unpleasant smell, the sight of putrescent matter, or even the recollection of a nauseous idea, will excite the stomach and œsophagus into reversed action. And thus also in the arterial system, as will appear hereafter, the vessels may be excited into reversed action with great facility, by a variety of apparently slight and transient causes.

29. It is worthy of observation also, that as in the arteries the reversed action progresses in trains of contractions from their minute extremities towards their large diameters, so in the veins and lymphatics the natural action proceeds in the same way. Here then is a perfect analogy between the natural action of the veins and absorbents, and the morbid action of the arteries. To say, therefore, that the muscular fibres of the capillary arteries are incapable of being excited, at least occasionally by any causes, into the same manner and direction of

action, which the veins and lymphatics, from ordinary causes, habitually assume, would be a flagrant solecism in logic, and altogether unreasonable in the nature of things.

30. It is no objection to the reversed action of the arterial system, to say that it is contrary to the course of the blood, and contrary to the natural action; for the same objection will apply equally to the reversed action of the intestinal canal, and the other portions of the vascular system; and yet it is manifest to every observer, that this action daily occurs in them. The idea, however, of its being an unnatural action, seems to have prevented the discovery of its recurrence in that degree and extent which the arterial system evidently admits, and which we believe does most certainly happen in certain conditions of the body.

31. If the physiologist had never witnessed the act of vomiting, he would not perhaps have discovered that the œsophagus, the stomach, and the intestines were susceptible of a motion so opposite to the usual course of the aliment, and so much at variance with the order of nature. And the supposition that they were so would appear to him as unnatural and improbable as the doctrine of this kind of action in the arteries may now appear to others; unless indeed he were to sacrifice his prejudices on the altar of reason, and infer the truth from the analogy of their structure and manner of action with that of similar organs of motion in the animal machine. But we constantly observe this morbid action in the alimentary canal in vomiting, in cholera morbus, and in the iliac passion; we, therefore, know that it occurs and constitutes some of the most formidable disorders of that system. However unnatural it may appear at first, we grow familiar with it day by day, and our surprise at the idea of the course of nature being reversed, ceases with our conviction of the fact: and when those obstacles are removed which have prevented us hitherto from acquiring a clear perception of the nature of *disorder* in the arterial system, and from comparing it with the derangement of natural order in other parts

of the human body, we shall have no hesitation in adopting the principle of reversed action, as a plain and obvious truth.

32. When we consider that the reversed action of the system of capillary arteries, although opposed to the natural current of the blood, can never be such as to reverse the motion of that fluid in the arterial system—that although it counteracts the healthful circulation, yet is ineffectual in preventing it, or in changing its course—that the motion of the blood is ever under the control of the superior power of the heart and large arteries so long as they continue to perform their office—and that the reversed action, although it is inconsistent with health, is not incompatible with the functions of life,—every difficulty and every objection will vanish. And when we consider further, that the reversed action of the arteries is in its very nature a morbid action, and that the essence of disorder in the physical, as well as in the moral world, consists in a direct opposition to the established order of nature, we shall be furnished with an additional argument in favour of the principle we have adopted. It is from not resorting to this plain and obvious principle, that moralists and divines have been unable to explain the cause and the nature of much of the moral evil in the world: and from inattention to the same principle, physicians have failed in accounting for many disorders in the human body. Without a knowledge of the reversed action of the alimentary canal, half its disorders would be involved in obscurity. Without a knowledge of the reversed action of the lacteal and lymphatic system, so ably illustrated and explained by Dr. Darwin, most of its disorders were inexplicable. And without a knowledge of the reversed action in the arterial system, its most formidable disorder has hitherto remained an inscrutable mystery.

33. I lay it down then as a general principle, that all the muscular tubes in the body do necessarily, from their very structure and organization, admit of motion in either direction; that is, the circular fibres, which chiefly contribute to move the contents of these vessels, may be excited into suc-

cessive action in opposite directions at different times—that the tendency of natural and salutary stimulants is always to excite the natural and healthy action in the ordinary course and direction of the fluids, and thereby to promote the health and vigour of the animal economy—that the tendency of unnatural and hurtful stimulants is always to produce an unnatural and reversed action of all the vessels; and that this is a wise provision in the constitution of the human body, to guard it from the effect of hurtful powers, to prevent them from being taken into the body, or, when in the body, to expel them from it.

34. Thus, for example, to show the extent and operation of this principle in the human constitution, suppose a man to be travelling upon a narrow path in an unfrequented wilderness—the silence of the place, and the darkness of the scene, preserve his organs of sense, his ears and his eyes for example, in a state of quiescence and inaction, by which, however, they become more susceptible to the impressions of sounds, and of objects. If he hears the sound of music before him, it excites the organs of hearing into natural, vigorous, and healthy action, his whole body is animated, and his steps are accelerated in a forward direction. But if instead of music he hears before him the howling of carnivorous animals, his organs of hearing are excited into unnatural, unpleasant, grating, painful action: they receive an impression, a sensation and a motion directly the reverse of the natural one; his forward motion is instantaneously arrested, and his steps are reversed in an accelerated degree. But suppose again that by a courageous effort of his reasoning power, he resists and overcomes the natural effect of these impressions on his organs of motion, he arms himself, and proceeds on his way; the beasts of prey, at the sound of his steps, flee into the wilderness. If then a beautiful and cultivated prospect opens to his view, his *organs* of *vision* are excited into pleasant, natural, and invigorated action. But if, on the contrary, he is precipitated into a dark and dis-

mal swamp, and the witch of Endor, or the ghost of Hamlet, rise up before him, his organs of vision are excited into disagreeable, painful, and reversed action; his countenance becomes pale, and the vessels on the surface of the body at least, by sympathy of action, assume a momentary reversed motion, and expel the blood from the vessels of the skin. He retraces his steps in a backward direction, and escapes the visionary danger.

35. But again, if in the progress of his journey this person meets with a garden of delicious fruits, his *taste* is feasted by a delightful, grateful, and natural action of that organ; the natural secretions of the mouth and of the stomach are increased, his digestion is active, and his body is strengthened. If, on the contrary, instead of a garden of fruits, he finds before him a dead carcase, which the wild beasts of the desert have partly devoured, the putrid effluviæ derange his organs of smell; they assume an action the reverse of the natural and pleasant one, and a sickly sensation is produced. By natural sympathy or association of motion the absorbents of the mouth are reversed in action—they pour out their saline and mucous matter—the beginning of reversed action in the stomach is perceived by nausea, or a disposition to vomit, and he turns from the object with disgust. If, however, he resists this first aversion, proceeding from the commencement of reversed action, and attempts to eat a portion of the putrid carcase, as soon as it touches the internal parts of the mouth reversed action involuntary expels it; or if by forcible means it be brought to the gullet, a more powerful retrograde action meets it there, and drives it back. If here it is possible for the voluntary power to overcome the natural impression of this putrid food, and it is swallowed into the stomach, it meets with no better reception; reversed action is instantly induced in that organ, and it is vomited up. But if it even remains in the stomach, and afterwards takes the natural course of the aliment, the mouths of the lacteals are excited into a reversed action similar to that of the mouth, gullet and stomach, and refuse to absorb it, leaving it to pass out of the body

by the natural emunctory. But suppose the lacteals disobedient to the hurtful impression of this putrid matter, and they carry it into the lymphatic system: here it produces its accustomed impression, which is a tendency to reversed action, that it may be prevented from entering the circulation of the blood, and that it may be expelled by some other course. The lymphatics are accordingly excited into reversed action, and by the preternatural and hurtful stimulus of the offending matter, either the muscular fibres of the valves of these vessels are contracted, by which the valves are partially opened, or by the innumerable anastamoses of the system of absorbents and lymphatics, the poison escapes by the branches which lead to the intestinal canal, the urinary organs, or to the skin. And, lastly, if by possibility some of the putrid matter be carried from the absorbents into the circulating system unchanged, it excites a reversed action in the sanguiferous system, so far as the organization of that system admits of this mode of action. It palsies all the powers and functions of life, and it leads to death and to the grave; by which the body becomes assimilated to the putrid carcase it first encountered, and the effect of which it had ineffectually resisted by all the organs and all the powers of the animal machine.

36. I come now, in the third place, to consider the *convulsed action of the arteries.* This consists, not in any degree of increase or diminution of their natural action, nor in any modification of their reversed action; but it consists in the irregular contractions of their muscular fibres; that is, the muscular fibres, instead of contracting in regular trains in an order of motion either from the heart or from the minute extremities of the vessels, contract indiscriminately, without relation to time, succession or order of motion. The nature of this kind of convulsive action is elegantly demonstrated in the moving fibres of the subcutaneous muscles in dying animals after the removal of the skin. The indescribable versatility and irregularity of contraction in the separate fibres of these muscles can be compared to nothing in nature so aptly as to the

versatile and irregular motion of the streaks of light in the Aurora Borealis. This is that kind of action which I would strictly call convulsive action in muscles.

37. On this subject, however, it is to be remarked, that there is an essential difference between the organization and manner of action of the muscles which envelope the vascular parts, and of the other muscles of the body. In the arteries, the veins, the intestines, and the lymphatics, for instance, the muscular fibres are so loosely connected with each other that they act in succession one after the other: when one of the fibres is excited into contraction, it is followed by the next contiguous one, and so on in progression in either direction. But in the muscles of locomotion, for example, the fibres are so intimately united, that they naturally contract at the same instant. When one of the fibres is excited into action the remainder also contract. In the one instance then the natural action of the separate moving fibres is successive, and in the other simultaneous. This appears to be the reason why convulsive action in the locomotive muscles is always attended with great pain in the substance of the muscles; for the irregular contraction of the separate fibres cannot take place without exerting a mechanical violence on the organization of the muscles, and on the nerves of sensation. And for the same reason it would seem that the circular fibres of the vascular system admit of convulsive or irregular action, without painful sensation being always induced; for in their natural condition they are capable of acting in succession, or at different instants of time, one before the other.

38. There is also another essential difference between the action of the muscles of the vascular system, and the muscles of locomotion. In the one the contractions are involuntary, and in the other they are under the habitual control and caprice of the will. In the one the regularity of natural action is preserved by the influence of uniform and continued excitement from material causes; in the other the natural action is induced for the most part by the voluntary power. But in convulsive

action of the locomotive muscles, the voluntary power is suspended and overcome by other exciting causes, and the contractions become necessarily irregular and unequal, because there is nothing to govern them or to preserve their natural regularity. Whereas in the muscles of the arteries, for instance, there is no voluntary power to loose; and, notwithstanding their exciting powers may be changed or increased, they may still preserve their natural regularity of action. Hence we perceive the reason why convulsions occur so much more readily and frequently in the voluntary than in the involuntary muscles; for in general convulsions of the locomotive muscles, neither the action of the heart and arteries, nor of the alimentary canal, nor of the lymphatic system, appear to partake of the convulsive action, or to be much disturbed in their natural motion.

39. Convulsive action in the muscular system can never consist in the mere force or frequency of natural action, according to the common opinion on this subject: for this force and frequency of contraction can be induced in the greatest imaginable extremes, without, at the same time, producing convulsion or disorder in the body: as, for example, in violent running, or in forcible exertion to extricate oneself from danger or durance. Here the muscles are actuated by the greatest preternatural degree of force and frequency, and even with great irregularity of action in respect to each other, as is manifest from the various contortions of the features of the body; and yet no one will call this a fit of convulsion. There is no painful sensation from the muscular contractions, evidently because the fibres of each muscle act simultaneously, and not irregularly, or at different instants of time. Notwithstanding the general impression that the muscles act in convulsion with preternatural force, there is reason to believe that their action is less powerful than that which they are capable of exerting in the healthy state; for, independently of the state of debility which is held to be the predisposing cause of convulsive action, the irregula-

rity of contraction in the separate fibres of the muscles would seem to prevent them from exerting the same force, which would result from the simultaneous contraction of all the fibres: and this is conformable to facts; for there are instances where the tendon of achilles and the patella of the knee have been rent asunder by the mere force of natural action in the muscles attached to them: but no such indications of force and violence have ever happened from convulsive contractions.

40. Although it is possible that convulsive action may occur in the arterial system, as well as in the other muscular parts of the body; yet, from the preceding considerations, and for other reasons to be mentioned hereafter, it does not appear to me probable that it often takes place, except, perhaps, in the last retreat of life, after it has deserted the other parts of the muscular system. Whatever force and frequency of action may exist at times in the arterial system, that, as we have seen, cannot be, in itself, an indication of convulsive motion: and in consequence of the uniform action of the arteries, without the influence or intervention of the will, they are less liable to irregular action than other muscles generally are; as in the apparently universal commotion of the body in general convulsion, they appear to escape almost unmolested.

41. If, moreover, we suppose the arteries liable to convulsive action for any long continued period of time, there can be no reason assigned why this convulsive action should not be communicated to the heart, and also to the other systems of muscles in the body. There is, indeed, so intimate a connection and sympathy between the heart and arteries, and they are both so much under the influence of the same exciting power, to wit, the blood, that it can scarcely be conceived that the arteries should be affected with convulsions, and yet the heart continue its natural action. This, however, must be supposed to occur, if we admit the existence of convulsion in the arteries; for we cannot conceive the existence of convulsion in the heart, without the idea of its immediately interrupting the cir-

culation of the blood, and the functions of life. When we consider also that the arteries pervade every part and every muscle of the body, and constitute no inconsiderable portion of their very substance, it is not conceivable that so large and extensive a system of muscular fibres could continue in a state of convulsion without communicating the disorder to the other muscles of the body more susceptible than themselves of convulsive motion. But in those states of the body which manifest the most considerable disturbance in the circulation, and in which alone this convulsive action can be conjectured most probably to happen, as in fever and inflammation, there is nevertheless no peculiar or prevailing disposition to general convulsion discoverable in the body.

42. Having thus particularly described all the different kinds of action of which the heart and arteries are capable, it is proper now to remark, that fever does not consist in the mere increase or diminution of the natural action in the sanguiferous system; because that may take place in the greatest extremes without at the same time producing the characteristic symptoms of fever; and because it is altogether insufficient to account for the peculiar phenomena of the disorder. Neither does fever consist in the convulsed action of the arteries, because in many states of mild and almost imperceptible fever there is no evidence or appearance of such action; nor is it credible that in these cases there is a convulsion of the blood-vessels. I shall contend, therefore, that fever does necessarily consist in the reversed action of a portion of the arterial system; and that this constitutes essentially the morbid action of fever. There is abundant reason to believe that this reversed action always occurs in every fever, that without it no fever can exist, and that it constitutes the proximate cause of the disorder.

43. I have said that fever does not consist in any degree of the natural action of the sanguiferous system. Thus, for example, the action of the heart and arteries may be increased by athletic exercise of the body, and also by other causes, to a degree of

force and violence unequalled in fever, and yet no fever be induced thereby. Instead of the blood receding from the surface of the body, it fills and swells the vessels, and they become more turgid than in health. Instead of a dry and parched skin, a profuse perspiration occurs. Instead of nausea, and vomiting, and loathing of food, no sickness takes place, and the appetite is increased. Instead of pain and weakness being induced, health and strength are retained. In short, none of the characteristic appearances of fever accompany the most excessive degree of natural action that can be produced in the heart and arteries, evidently because the natural action is unchanged, and because no modification or degree of natural action can in itself constitute fever.

44. Thus also the action of the heart and arteries may be diminished by abstinence, by disease, and by old age, far below that degree which occurs in the ordinary state of fever; and yet the body will be entirely free from fever; and for the obvious reason that no degree of debility or diminution of natural action can constitute the morbid action of fever.

45. Notwithstanding the difficulty, nay, even the impossibility of accounting for the symptoms and appearances of fever on the principle of natural action in the sanguiferous system, yet this seems to have been the universal error of medical philosophers. This principle pervades all the most celebrated European systems and theories—It is taught in their universities—It is promulgated in books—It is handed down from one to another, and no one takes the trouble to inquire whether it bears on its front the marks of truth. Although it is evidently insufficient to explain the morbid appearances of fever, yet no one has extended his views to the discovery of another, and a better principle, on which to found the doctrine of the disorder. The theories of Cullen, of Brown, and of Darwin, are all equally founded on the supposition that a mere increase or diminution of natural action in the heart and arteries constitutes the essence of fever. They endeavour to account for a *pre-*

ternatural disturbance in the circulation, on the principle of *natural* motion; and to explain a *morbid* condition of the body as consistent with *healthy* action—The *disorder* of the arterial system is not distinguished from the *natural order* of motion—And *sickness*, according to them, is produced by the same manner of action in the blood-vessels as *health*. They have, consequently, all failed in their attempts to investigate the proximate cause of fever, or to give a rational explanation of its phenomena. And although theory after theory, and system after system have fallen into ruin as fast as they were reared, still every succeeding writer has continued to build on the same insufficient foundation. This, I have said, appears to have been the universal error of *European* pathologists, because they have all embraced it. But in this country the science of Medicine has been emancipated from that error by the genius and talents of Dr. Rush. His penetrating mind early discovered the imperfections of the prevailing doctrine, and the fallacy of the principle on which it rested. He was accordingly led to conceive the idea of a preternatural kind of action existing in fever, essentially different from the natural action of the arterial system; and from the supposed analogy between the disordered state of the blood-vessels in this complaint, and the irregular action of the muscles of the body in convulsion, he has erroneously placed the proximate cause of fever in a convulsive action of the arteries. Although he is evidently correct in the general principles of preternatural or morbid action, still he has erred, as I conceive, in the application of that principle to the manner of action in the arterial system. But, notwithstanding he may not have arrived at the truth, yet he has opened the way for its discovery, by dissipating the prejudices of error, and pointing out the true path of investigation.

46. This leads me to remark in the next place, that as fever does not consist in the natural action of the arterial system, so neither does it consist in the convulsed action of that system. This doctrine of convulsion in the blood-vessels is attempted to

be supported by a pretended analogy between fever and convulsions in the nervous or muscular system in general. But there is no such particular and characteristic analogy existing, as is competent to establish the identity of action in the two disorders. On the contrary, the analogies which do occur are, for the most part, such as are common to most diseases, and such as are not sufficiently specific to distinguish one disease from another. Thus, for examples: Are convulsions preceded by debility? So is fever, and so also are most other diseases. Do convulsions, under certain circumstances, affect the brain? So undoubtedly do certain states of fever—so do certain other diseases—so do certain medicines—and so do many other causes: Are there certain convulsions which do not affect the brain? So neither do certain fevers, and so neither do most other diseases. Are there local convulsions? So also there are numerous local disorders. Are there certain grades in convulsions? So there are necessarily in all diseases. Are convulsions most apt to occur in infancy? So also are a variety of other complaints. In this way we might go on and enumerate circumstances and symptoms which are common to a vast number of disordered states of the body. But these considerations could have no weight in establishing the sameness of the proximate cause in all those disorders; and they do by no means prove the similarity of febrile and convulsive action.

47. If there be any appearances of convulsive action in the muscles during fever, as the tremors in the cold state of intermittents, and the occasional supervention of general convulsion in other fevers, it is to be remembered that the seats of the two disorders are in different systems of the body, which are subject to different kinds of morbid action from the same exciting causes. Accordingly, while convulsive action in the muscles of locomotion happens, a very different kind of action may at the same time occur in the arteries in fever: For we know that those muscles admit of no other morbid action than convulsive; but the arteries are capable of reversed action, which is more easily

induced, and by less powerful causes than convulsive action. It is, therefore, no argument in favour of fever being a convulsion of the arterial system, that convulsion is induced in another system of muscles.

48. I shall readily admit, that in certain states of fever the irregularity of action in the pulse is such as to resemble, in some measure, convulsive motion in other parts of the muscular system. But I do not thence concede that it constitutes the proximate cause of the disorder; for although it be granted, that the sanguiferous system, and particularly the heart, is, like all other muscular parts, capable of convulsive action, and that this convulsive action does sometimes take place in the highest grades and last stages of fever, yet it does by no means follow, that therefore convulsive action always exists in fever, and operates as its immediate cause. On the contrary, in the ordinary states of fever, and particularly in the mild grades of it, where the morbid action of the heart and arteries is not irregular, and is scarcely to be distinguished from the healthy action, there is no resemblance, in the movements of the sanguiferous system, to convulsive action. If, therefore, convulsion of the heart and arteries at any time happen, it is rather to be considered as an accidental circumstance, or a consequence of fever, than as its cause; for it cannot be supposed to be its cause unless it occurs always, in all fevers. But in the greatest number of febrile cases the action of the heart and arteries is as uniform and regular as in health. To say, therefore, that in such cases of mild and almost imperceptible fever, there is a convulsion of the blood-vessels, would be like saying, that nausea is a convulsion of the stomach.

49. It now only remains for me to show that the proximate cause of fever is the reversed action of a part of the arterial system. Here I cannot avoid noticing an important observation of Dr. Rush, which is strictly applicable to this part of our subject, and tends to confirm the truth of my doctrine. It is, that the essence of all diseases " consists in irregular action, *or in*

" *the absence of the natural order of motion*, produced or invited " by predisposing debility." Agreeably to this fundamental principle, which I consider as having been misapplied in the case of morbid action in the arterial system, I shall contend, that in all those organs of the body, whose natural action consists in a simultaneous contraction of their fibres, as in the individual muscles of locomotion, *disorder* consists in their irregularity of action, and that hence proceed convulsions. But that in all those organs whose natural action consists in a successive *order* of contractions, as in the vascular system, *disorder* consists most commonly in the absence or *change of the natural order of motion;* and that from thence proceed vomiting in the alimentary canal, and fever in the arterial system. The truth of this position is to be inferred from all those considerations which I have already mentioned, as well as from all the symptoms of fever, which are next to be considered.

50. The most conspicuous phenomena of fever, and those which always occur in the disorder, are, *First*, A languor, or sense of debility, a sluggishness in motion, and some uneasiness in exerting it, with frequent yawning and stretching. *Secondly*, A paleness and sallowness of the skin, a shrinking of the features, and a diminution of the bulk of every external part, as if sudden emaciation had taken place; a disappearance of the fulness of the blood-vessels on the surface, and an apparent retrocession of the blood to the internal parts of the body. *Thirdly*, A sense of oppression at the præcordia, a laborious respiration, and an unusual excitement of the heart and large arteries, manifested most commonly by a preternatural frequency, and sometimes by an oppression of the pulse.

51. The first of these symptoms are universally admitted to denote debility, which always precedes fever, and constitutes its predisposing cause. The second have been variously interpreted. Dr. Boerhaave ascribed them to a lentor or viscidity of the blood, and this he supposed to be the proximate cause of fever. Dr. Hoffman and Dr. Cullen conjectured them to de-

note a spasm of the extreme vessels; and this constituted their proximate cause of the disease. Dr. Darwin considers them as the effect of a torpor of the capillaries of the skin, which forms a material link in his chain of the theory of fever; while Dr. Brown and Dr. Rush appear to view them as the consequences of mere debility, or deficient excitement in the system.

52. It would be superfluous at this day to attempt to refute the opinion of Dr. Boerhaave. Suffice it to say, that no such lentor or viscidity of the blood as he describes is found to happen: and his theory is now universally exploded. Almost equally superfluous would it be to controvert Dr. Cullen's imaginary doctrine of spasm; for there is no evidence of the existence of such a state of the blood-vessels, and if there was it would be insufficient to explain the subsequent symptoms of fever. His theory is accordingly also very generally rejected. The torpor of the capillary vessels, supposed by Dr. Darwin to occur in fever, very probably exists in the state of predisposition; but it cannot be presumed to be the proximate cause of the disorder, unless it continues during its whole progress, which, however, it is not alleged to do. The conjectures of Dr. Brown and Dr. Rush are as unsatisfactory as all the others we have mentioned; for extreme debility may exist in the body, and yet those appearances which take place in fever are not produced by that debility. There will be no "retreat of the blood towards the viscera," according to the remark of Dr. Rush. The vessels under the skin will not shrink and disappear, nor will the action of the heart and the respiration be oppressed.

53. To what then, I ask, are those peculiar symptoms of fever to be ascribed? The answer is obvious. They can be ascribed to no other cause than the reversed action of the capillaries and small arteries. This morbid action, by its recurrence in fever, and its continuance during the disorder, is competent to produce all its various modifications; and by means of it alone can we account for the various appearances and symptoms

of the complaint. We accordingly find no difficulty in explaining the third class of phenomena we have mentioned; to wit, the sense of oppression in the body, and the unusual excitement in the action of the heart and large arteries. These are the natural and necessary consequences of the reversed action of the extreme parts of the arterial system, giving a preternatural resistance to the free circulation of the blood, and accumulating it in unusal quantity in the system of large vessels in the interior parts of the body.

54. Let us now see how our doctrine applies to the different stages and states of fever. Debility, either direct or indirect, I have said, is the predisposing cause of all fever. This debility, in the vascular parts of the body, whose muscular fibres habitually act in succession to each other, always produces a disposition to that derangement of the natural order of motion which happens from their acting in a reverse direction. And this reversed action is always more easily induced in the vascular organs, and by less powerful causes than convulsive action: hence it generally precedes, and seldom or never follows convulsive action. The action of the heart and arteries then, in common with other muscular action, becomes weak in the incipient state of fever. It ceases to extend its influence to the system of capillary vessels—They become doubly debilitated, both from internal and external causes—Their trains of contractions are no longer governed by the impulse of the blood from the heart—They yield to the operation of other stimuli, which now produce a greater relative effect—Their circular fibres are excited into action at their minute extremities; and the contractions necessarily follow each other in succession up the course of the arteries, in a direction opposite the natural order of motion. In consequence of this reversed action of the capillaries, the circulation is retarded, the secretions are diminished, the skin becomes dry, the extreme vessels lose their fulness, and the blood accumulates in the large arteries. This accumulation of blood produces a preternatural distension

of the vessels nearest the heart, and offers an uncommon resistance to the action of that organ. It becomes oppressed, and labours to overcome the unnatural obstacles opposed to its power, in the same manner that its action is increased from the accumulation of blood produced by the pressure of the muscles in violent exercise. Its motion is accordingly accelerated. Its contractions are usually more frequent than natural, often irregular, and sometimes depressed. The respiration becomes difficult and laborious, and the functions of life disordered. This commotion in the circulation is commensurate in degree and duration with the degree and duration of the reversed action in the extreme vessels. And this, according to the various conditions of the system, under all the varying circumstances of animal life, produces all the different states of fever.

58. In the common *intermittent fever*, the reversed action of the capillaries produces first the cold stage, and afterwards, by preternaturally exciting the action of the heart and large arteries, gives rise to the hot stage and its attendant symptoms. This fever, more than any other, is accompanied with a sensation of chilliness, because, as I suppose, the frequent exposure of the body to alternate heat and cold has no small share in producing the predisposition to it in the system, and the sensibility of the skin is left either unimpaired, or perhaps preternaturally increased. Besides, as this exposure of the body to heat and cold is for the most part periodical, owing to the greater heat of the day than of the night, it may induce a corresponding periodical habit of alternate excitement and debility, which will explain, in some measure, the periodical recurrence of the paroxysms. It is to be remarked also, that as those causes which predispose to this fever, act primarily and principally on the external surface of the body, the capillaries of the skin are first affected, and the capillaries in the other parts of the body secondarily by association. And this too may explain why the affection of the skin, that is, the coldness and chilliness in the commencement, and the heat and redness in the progress of

the paroxysm is greater than in most other fevers. With this also is to be connected the consideration, that, as in this fever the surface of the body is the part primarily affected, there is no great degree of debility induced in the whole system, and the weakness of the action of the heart in its commencement is rather the consequence of association with the capillary vessels than of general debility or specific weakness of that organ; and that hence it is soon excited into such vigorous action by the stimulus of the accumulated blood, as to overcome the reversed action in the capillaries, and to extend the natural excitement and wholesome action to the surface of the body, when the paroxysm subsides, and the fever terminates periodically. In some cases of malignant intermittents, however, attended with great general debility, the action of the heart continues so much enfeebled and oppressed, as to be utterly incapable of overcoming the reversed action of the capillaries; and death consequently happens in the cold stage. In these cases it is observed that no sensation of chilliness occurs, and the large arteries are found, on dissection, to be surcharged with blood, similar to what happens in other malignant fevers. These circumstances indicate at once the great degree of debility, and the great degree of reversed action subsisting in those fatal conditions of fever.

56. In the simple *inflammatory fever* which happens most commonly in the cold season of the year,—not in consequence of periodical exposure to alternate heat and cold, but in consequence of long-continued exposure, first to cold and afterwards to heat,—the capillary vessels are more generally affected. Not only those of the skin, but the more deep-seated ones become immediately debilitated; and accordingly the reversed action is at first more extensive than in intermittents, and afterwards more permanent. This fever, therefore, assumes a more continued type, and remits only in consequence of the general habit of the body, with respect to periodical excitement and debility from the difference of temperature to which it may

have been habitually exposed, from the coldness of the day, and the warmth of a bed at night.

57. In *milignant fever*, which happens when the body has been exposed to a continuance of excessive heat and moisture, and where the exciting causes are of a deleterious nature, there is always great and universal debility. In this case the action of the heart becomes often suddenly oppressed, and its feeble power sinks under the accumulation of the circulating fluid produced by the reversed motion of the capillaries. Its force is insufficient to subdue the morbid action in the extreme vessels, and death consequently ensues.

58. The limits of this essay will not permit me to be more particular in the application of my doctrine to the different states and circumstances of fever. Enough has been said to show that it will furnish an ample, and, we trust, a satisfactory explanation, of the most important and characteristic phenomena of the disorder.

59. Thus, I have given a rude sketch, an imperfect outline of a theory of the proximate cause of fever. As it differs materially from all other preceding theories adopted by writers on medicine, it may be properly called the *reversed theory of fever*, to distinguish it from the *metaphysical* theory of Stahl, the *mechanical* theory of Boerhaave, the *spasmodic* theory of Hoffman and Cullen, the *putrid* theory of Pringle, the *sthenic and asthenic* theory of Brown, the *sympathetic* theory of Darwin, and the *convulsive* theory of Rush.

60. On the subject of the proximate cause of fever I have ever entertained but one opinion, since I began to study the book of nature, instead of the speculations of authors. None of the prevailing opinions satisfied my mind, or appeared sufficient to explain the nature of the disorder. I accordingly embraced the doctrine I have delivered in the early period of my studies, and have been more and more confirmed in the conviction of its truth by subsequent reflection and observation. By adopting this theory we avoid the insuperable difficulties of

Dr. Cullen's hypotheses, that require for their support, at every step, the assumption of an indefinite principle, an imaginary "*vis medicatrix naturæ*," which truly is sufficient to explain, at pleasure, any thing or nothing. We moreover escape the dilemma of Dr. Rush, in his attempt to assign the reason "why excess in the force or frequency of the action of the "blood-vessels should succeed debility, and be connected for "days and weeks with preternatural debility in the muscles, "nerves, brain, and alimentary canal," for which the Doctor has utterly failed to account in a satisfactory manner. And we are extricated from the inconsistency of all other theorists in their fruitless labours to explain a disordered, unnatural, and morbid state of the body, on the principle of natural order and healthy action in the economy of life.

61. If I should fail in establishing this theory of fever, it will be some consolation to me to reflect, that I shall only share the common fate of all my predecessors in medicine: and with them I shall enjoy the satisfaction of having laboured *to discover and to promulgate new truths, rather than to confirm and to propagate old errors.*

To the Editors of the Medical and Philosophical Journal and Review.

GENTLEMEN,

The Author of the following Lecture, which is now sent to you for publication, had no idea, at the time it was written, that it would ever appear in print. This observation is not made to excuse any errors which it may contain, but to prevent a charge of plagiarism for not having marked every thing which is borrowed with inverted commas. These were thought unnecessary at the time the piece was written, as no person but the Author could have seen them, and his memory did not afterwards enable him to discriminate between what was borrowed and what was original. In the writers he quotes, most of the facts and opinions which are brought forward may be found; but as these are not generally known, they may not be unacceptable to your readers.

Yours, &c.

J. A. SMITH.

A LECTURE *introductory to the second Course of* ANATOMICAL INSTRUCTION *in the College of Physicians and Surgeons for the State of New-York; delivered in that Institution, on Friday, the* 11*th of November*, 1808, *by* J. AUGUSTINE SMITH, *of the Royal College of Surgeons, London, and Professor of Anatomy and Surgery in the University of the State of New-York.*

NO flowers of rhetoric, no flights of fancy, Gentlemen, grace the lectures of the Anatomist. Demonstration, his cold, his creeping, his constant companion, conducts her votaries to the temple of knowledge, by a route safe and certain, but unadorned. Her train is, indeed, seldom numerous; for men of brilliant imaginations and lively fancies scorn her slow and timorous progress, and soar at once into the regions of hypotheais. But too often they find they have risen on waxen pinions, which, melting before the sun of truth, they fall into an ocean of oblivion or contempt. To avoid a similar fate, we shall never wander from our guide, in tracing the peculiarities of structure which distinguish the various races of men; a subject eminently calculated to interest the man of science of whatsoever denomination.

Before we begin the inquiry, it will be necessary to define a word which will frequently occur—the word *race*, as applied to the human species. It is not intended to imply by this word an original or radical distinction among men, but merely to express the fact, that differences do exist. These distinctions are now so obvious as to strike the most careless observer, and so great as to confound the most acute inquirer. While by one class of philosophers they are attributed to climate and other secondary agents, another, conceiving these causes inadequate to the effect, boldly assert the existence of distinct varieties of men. Before I examine these different opinions, I shall first fairly state the facts involved in the discussion. I shall not, however, bring forward every minute peculiarity which anatomists have detected, and which might be thought to exist in the brain of the dissector only, or to have arisen from some accidental circumstance; but I shall confine myself principally to those varieties of structure which are too obvious to be delusive, too constant to be fortuitous. I shall endeavour to prove that the anatomical structure of the European, whatsoever may be the cause, is superior to that of the Asiatic, the aboriginal American, and the African, or, at least, that it is farther removed from the brute creation. But I shall principally contrast the European and the African, because, being placed at the opposite extremes of the scale, the differences between them are more numerous, and more strongly marked. Before we proceed to compare the various races of men with each other, we must point out the differences between man and other animals.

Naturalists have arranged all living substances under two grand denominations, the animal and vegetable kingdoms. These they have subdivided into classes, orders, genera and species. Yet these divisions are the work of man, not of nature, for nature forms individuals only, and so gently does she glide from one class, genus, or species to another, so faint are the lines that separate even the kingdoms from each other, that it is impossible to say where the one ends and the other begins. "The water polypus," says Buffon, "may be considered as the "last animal or the first vegetable." Yet, though the descent is constant, it is not uniform, for man is clearly distinguished from all other animals by his anatomical structure; some, however, resemble him much more nearly than others: among these the simiæ or apes hold the first rank, and among the apes the ourang outang stands next to man.

It was for a considerable time supposed that the human brain exceeded that of any other animal in size, but some of the cetaciæ far surpass man in this respect. It was then imagined that, compared with his body, his brain was larger than that of other animals; and so it is with regard to the whale, the bulk of whose body is yet larger than his brain, compared to that of the human subject; but not so with respect to some of the smaller animals, (the Canary-bird is one) whose brain, compared with their body, is larger than ours. Lastly, it has been found that of all animals hitherto dissected, the nerves of the human subject, compared with his brain, are the smallest. In the lowest order of animals there is no distinct brain, but merely a few nervous chords. But man differs from other animals more in the form than in the capacity of his cranium. This form is most conveniently measured and expressed by what is called the facial angle. This angle is formed by the intersection of two lines at or parallel to the inferior part of the nostril, one being drawn from the most projecting part of the forehead until it strikes the edge of the incisior teeth of the upper jaw, the other from the inferior part of the bony canal of the ear until it meets the other at the place above mentioned. Sometimes it is called the facial line; in which case the lower line is always supposed horizontal, and the inclination of the other to it is meant. There is a very remarkable coincidence between the obtuseness of this angle and the intellect of the animal; but there is one source of error in its application, which must be pointed out. In all animals whose brain has a bony covering, there is a separation of the two plates or tables of the skull, as anatomists term them, just above the nose, forming what are called the frontal sinuses. These sinuses vary considerably in size, and it is evident where they are large they would cause a considerable projection of the forehead, without a corresponding elongation of the brain. This is remarkably the case with the elephant,* whose facial angle is consequently very obtuse; and hence probably the great reputation he has acquired for intellect; his name, in the East-Indies, implying that he possesses reason.

In man this angle varies from 70° to 90°.† In the ourang outang it is 67°; Sapajou 65°; Talapoin monkey 57°. In the Mandrils, which are the most mischievous of the monkey tribe, it is only 30°. In birds it is much more acute than in quadrupeds, particularly in the goose; and hence, perhaps, the proverbial stupidity of that animal. Another circumstance to be attended to with regard to the heads of animals, is the relative proportion between the face and the cranium. Upon making a vertical section of the heads of the following animals, the proportions were, in the ourang outang one to two and a half; in the Sapajous one to two; in the Mandrils and in dogs they were nearly equal; whereas, in the horse the face is quadruple the size of the skull.‡

Man is the only animal that has a projecting bony chin. In those animals which most resemble him, it becomes gradually smaller, or more retreating, as the facial animal becomes more acute, until it entirely disappears.

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* Cuvier.

† The auther has had an instrument made by which the angle can be measured with the greatest accuracy.

‡ These proportions are taken from Cuvier's Comparative Anatomy.

The head of the human subject is placed far backward on the spine, or, as the fact is expressed by anatomical writers, the foramen through which the spinal marrow passes is more anterior in the human subject than in other animals, and is very nearly under the centre of gravity of the human brain; whereas, in other animals it is far behind it: Hence the erect position is so easy and natural to man, and so difficult to most other animals; none of them, the ape excepted, remaining long in this posture; and the ape, in this respect, as in most others, forms the intermediate link between man and quadrupeds. But, to compensate, the cause which renders the vertical position tiresome, makes the horizontal one easy and natural. Still better to adapt the inferior animals to this posture, they have in the neck a very strong ligament, termed vulgarly pax-wax, or white-leather, I believe, which, by its elasticity, nearly supports the head without any exertion on the part of the animal. This ligament,* strictly speaking, does not exist in the human subject; there is a slight appearance of it only. This fully refutes, if indeed such an absurd notion requires refutation, Lord Monboddo's opinion, that men, like other beasts, formerly walked on all fours, at which time too he supposed their posteriors were ornamented with tails, which, being constantly worn off by the unnatural custom of sitting down, children have latterally been born without them.

All apes are incapable of uttering articulate sounds, in consequence of two large sacs communicating with the larynx, the part where the voice is formed. Man is imitated in this respect by certain birds only. Apes have four hands, whereas man has but two: hence in Cuvier's arrangement, the former are called Quadrumana, the latter Bimanum. The superior extremity of the ape is proportionably longer than that of the human subject. I shall take notice of but one more peculiarity of the human subject, and which is more curious than any circumstance hitherto mentioned. There is attached to that part of the human intestinal canal called the cæcum, a small process or elongation, which, from that circumstance, and its shape, which resembles a worm, is called processus vermiformis cæci. There has never yet been an human subject dissected in which this was wanting, though a want of brains is by no means so uncommon, nor is there a well-attested instance on record in which it has been met with in an animal of any other species. There is an account of the dissection of an ape by Dr. Tysson, which, it is said, had one; but this is generally supposed to be a mistake, no other instance of it having occurred. The use of the part is not at all known to us.

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* Called by Anatomists Ligamentum Nuchæ: its only use is to give origin to some of the muscles at the posterior part of the neck.

Having thus shown the distinguishing characteristics of man as opposed to other animals, I shall proceed to the principal object of our inquiry, to point out the anatomical differences between the various races of men. Upon viewing an European and an African, the observer immediately contrasts the fair complexion of the one, with the sooty hue of the other; the straight or waving ringlets of the former, with the wool like hair of the latter; and the flat nose and thick lips, which mark that unfortunate portion of the descendants of Adam, are opposed to those more elegant features in the happier sons of Europe. At least we suppose the features of the European more elegant, though perhaps the African may not be of the same opinion, as it is said they consider a black skin as handsomer than a white one, for they supposed the first whites they saw devils from their horrible appearance. The white complexion has certainly one advantage over all others, as by means of its varying tints, some of the most exquisite and delicate emotions of the female mind are expressed, whereas a blushing African would be a phenomenon indeed.

The external surface of the body is covered throughout, except under the nails, by a thin insensible membrane, termed cuticle, epidermis, or scarf-skin. This membrane is transparent except where it is unusually thick, as on the palms of the hands and soles of the feet; and on these places it is somewhat white and opaque, and becomes perfectly so where it is exposed to much friction. As it is transparent, the rays of light pass through it to the next membrane, which is called corpus, or more generally rete mucosum, by which they are either reflected or absorbed, according to its colour: for it is this mucous substance which determines the colour of the body, except in those who are very fair. In the Europeans it is not only white, but somewhat transparent, which gives that clear appearance to their skins; in the Asiatic it is tawny; in the aboriginal of this continent it inclines to red; and in the African black.* According to Albinus the inner surface is of a deeper hue than the external. Under this lies what is called the cutis vera, or true-skin, which is of the same colour in all. We shall have more to say hereafter with respect to the structure and economy of these parts, when we come to speak of the common integuments; at present their colour is all we have to consider. The differences between the hair, nose, and lips of the African and European, require no particular notice, though it is a difficult matter to conceive how climate could have caused them.

We now proceed to consider the differences between the head of the African and the European. The facial angle† of the European is from 85 to 90°, of the Asiatic about 75 or 80°, the Indian of this continent 73½°, and the African only 70°.

This angle varies, however, slightly with age. In infants it is more obtuse than in grown persons; and hence the very interesting appearance which children exhibit. If the statues, busts, and impressions which remain of the Greeks and Romans were correct likenesses of the persons whom they represent, it would appear that a great degeneracy has taken place, which indeed many persons have suspected who were unacquainted with the difference in the facial angle. In those representations of the gods and heroes of antiquity which are Grecian, the angle is increased to 100°,* and in the Roman to 95°. It is supposed by some writers, that this form was given to the face to increase the appearance of intelligence in the countenance, for it is generally believed that the Grecian artists wrought from ideal models of perfection. This opinion, if correct, clearly proves they thought there was a connection between the features and the intellect of the individual;† and hence, perhaps, they made an owl the emblem of wisdom, the facial angle being remarkably obtuse in that bird.

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* The author has a preparation in which the colour of the rete mucosum and cutis in the African are shown.

† The author possesses a series of heads demonstrating the differences here mentioned.

I do not pretend to say that there is a certain and uniform ratio between the intellect of the individual and the obtuseness of this angle, as this was fully disproved by a Caribbean chief‡ of the island of Vincent, whose head is preserved in a museum in London. Certain tribes of Indians have a custom of altering the shape of the head, by compressing it between two boards when the child is young. The head of this chief had been so compressed, that the forehead, instead of being convex anteriorly, was concave; yet he was a man remarked for his information, and particularly for his knowledge in botany. But the forehead of the African is not only more retreating, but likewise narrower than that of the European; the African's occiput, or hind-head, is likewise smaller. From these two circumstances, his head is less capacious, other things being equal by about one-thirteenth.* The brain of the African is certainly firmer than that of the European. The spinal foramen of the former is more posterior, his teeth project more forward, and his chin is less prominent. The cheek bones of the Africans, according to Blumenbach, project more than those of the ancient Egyptians, which is the distinguishing mark between them.

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* Upon measuring the facial angles of the statues belonging to the academy of fine arts, this statement was found correct, or nearly so, it not being practicable to ascertain it exactly.

† Cuvier.

‡ An exact representation of the head of this chief is in the possession of the author.

The area of the face of the Calmuck, compared with his head, exceeds that of the European one-tenth. That of the Calmuck is surpassed by that of the African in the same proportion.† Upon measuring the arms of Africans and Europeans,‡ it was found those of the former exceeded those of the latter uniformly and considerably, where there was not a great difference in height in favour of the European. Negroes have been frequently found to possess an extra bone in the back. The bones of the leg of the African are bowed forward; nor are they articulated with the foot in the same manner with those of the European: hence they walk more awkwardly, and their feet are flatter. These are the principal differences between the European and the African. There are many others more minute and less important, with which I shall not trouble you. But in order that you may more distinctly perceive the difference for which I am contending, it will be necessary to take a concise view of those distinctions in which the African seems to approximate the lower orders of creation. The facial angle in the European is from 80 to 90°; in the African 70°; in the ourang outang 67°; and in other animals still less. The cranium of the African is less capacious than that of the European; other things being equal by one-thirteenth. In other animals which resemble man, the difference is still greater. The spinal foramen is more posterior in the ape than in the African, and is more posterior in the African than in the European. The proportional area of the face is greater in the ape than in the African, and greater in the African than in the European. The teeth of the first project more than those of the second, and those of the second more than those of the last; whereas the prominence of the chin bears an exactly inverse ratio to the projection of the teeth; and, lastly, the same proportion holds with regard to the comparative length of the arms in all three, as in the projection of the teeth.

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* This was the result of an examination made in London. The brain was taken out, and the capacity of the skulls ascertained by the quantity of water which they would hold.

† These proportions are taken from Cuvier. That there is a difference is demonstrable.

‡ Mr. White on the gradation of the human species.

Before I say any thing about the causes which may have produced these changes, I must point out the authorities for those facts which I have mentioned, and which I have not proved. Camper, a very celebrated German anatomist, was, I believe, the first who paid any attention to the subject, and it is to him we are indebted for the discovery and application of the facial angle. To Blumenbach, Mickel, Walter and Murray, I am likewise indebted for facts and observations. John Hunter was a man of too much anatomical knowledge, and too much observation, not to have noticed these differences in the form of the skull; and he had a series of heads, which Mr. White, well known as a surgeon, acknowledges first gave him the idea of a gradation in the human species. He has written a professed work on the subject. To Cuvier likewise, the first comparative anatomist of the age, am I indebted for some of the facts which I have detailed. I am acquainted with the writings of no anatomist who controverts them. Camper and Hunter were decidedly of opinion, which is indeed clear from Revelation, the best of all authority, that we are descended from one common stock. Hunter indeed supposed that this stock was originally black, because, he observed, that the descendants of white animals are always white; but that those of black are occasionally white, as albinos among the Africans. But this is a circumstance which never happens in their own country, according to the testimony of Park and other travellers; and arises probably from the colder climate into which they are transplanted. Hunter's first position is not correct, according to a case related in the Philosophical Transactions by a Mr. James Lind. In this instance it seems a white couple had a black child, which the good man thought was entirely a prank in dame Nature:—It is well her ladyship does not indulge in these vagaries in this sceptical age. Nichols thought, that as the Africans inhabit a very hot climate, and are of course obliged to go about in the night, they were made black, that they might stand a better chance to escape the wild beasts with which their country abounds.

If we have recourse to final causes there is no greater difficulty in solving this than any other phenomenon; but in this case that is impracticable, Revelation being clear and conclusive to the contrary. But authors, not reflecting that a bad argument weakens a good cause, have endeavoured to prop Revelation, which is in itself sufficient and complete, by referring the differences now observable among mankind, to the temperature of the climate they inhabit, and the various modes of life they pursue. That these causes are inadequate to the production of the effects ascribed to them I shall presently show. I must first examine a rule deduced from natural history to prove that we are all descended from a common stock.* The changes which climate and other external circumstances with which we are acquainted, produce in animals confessedly of the same origin, have induced naturalists to search for a criterion, by which the identity or difference of species of those animals which most resemble each other might be ascertained. Ray, and after him Buffon, adopted the following:—If the product of any two animals be capable of propagating, then they esteemed those animals to be of the same species, and otherwise not. Thus all the varieties of dogs are supposed to be from the same original stock, because, however crossed, the product is still capable of generating a dog; whereas, a mule,[*] the joint product of an horse and an ass, is incapable of propagating mules; hence horses and asses are supposed to have been originally distinct. But if this rule be just, then are not only all dogs, but likewise wolves and foxes, included in the same species, as was proved by John Hunter; and, on the contrary, the offspring of the common and Muscovy duck is incapable of propagation, though these two animals are certainly more alike than foxes and dogs. Buffon himself seems to think there were some exceptions to this rule; for in speaking of the sheep and the goat, he acknowledges them to be of different species, though their joint product is capable of propagation.[†] But in physics the case is precisely the reverse to what it is in law, for an exception, instead of proving, destroys the rule. Therefore no inference can be drawn from this pretended rule, when applied to the human race.

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* The author might have saved himself the trouble of saying any thing about this "pretended rule," as it is clearly an assumption incapable of proof. *Ed.*

We now come to consider the operation of climate and other external agents upon man; and in doing this I shall likewise examine the opinions of those who advocate their sufficiency to produce the effects which have been attributed to them. Lord Kaimes, in his sketches of man, brings forward some arguments to show the inadequacy of these causes to explain the distinctions of colour; for he was probably unacquainted with the other differences above detailed. He has been answered and criticised by the Rev. Dr. Smith, now I believe President of Princeton college, with considerable severity. In this dispute I think the Doctor has evidently the advantage of his Lordship, whose arguments, proving too much, prove nothing; for, from

[*] Some cases are mentioned by Buffon in which mul
but they are rare; the fact is of no consequence in the

[†] This observation by Buffon is taken from Mr. Wh

his account, the number of original pairs formed by the creation was indefinite.

In examining Dr. Smith's performance, I shall not make altogether so free with him as he has done with Lord Kaimes. But it is to his station in society that he is indebted for this forbearance, not to the merit of his work. The Doctor's zeal, though highly commendable, too far exceeds his knowledge; for I am afraid it will be found his anatomical information is by no means correct. Perhaps there is no subject of which philosophers are so generally ignorant as anatomy. Whether this proceeds from the comparatively few schools in which it is taught, and without a teacher it cannot be learnt, or whether it arises from the nature of the pursuit, which is apt to disgust fastidious minds, anatomical knowledge is almost exclusively confined to the profession.

Men who call themselves philosophers, or who wish others to consider them as such, too often suppose that this title is acquired not by a thorough acquaintance with a few sciences, but by a superficial knowledge of the whole: hence they frequently incur the ridicule of the world, by writing on subjects of which they are ignorant. But hear Dr. Smith on this subject.— " Mankind," says the Doctor, (p. 138) " will heap upon him " (the philosopher) the contempt he deserves for intermeddling " with a subject he does not understand."

The Doctor sets out with fair promises, which, at the conclusion, he thinks he has fulfilled. Authors, like the gentlemen of the bar, are apt to declare their own conviction of the justness of what they say; and like them, probably, persuade themselves into a belief of what they assert. In both cases we may give them credit for sincerity, without attaching much weight to their opinions. " On tracing the globe," says the Doctor, " from the pole to the equator, we observe a grada- " tion in the complexion nearly in proportion to the latitude of " the country." This is partially true, but the exceptions to it are great and inexplicable. Before they are stated, hear the manner in which he explains the action of the climate in producing this change of colour. He begins with the following position: "That the skin, though delicate and easily susceptible of impres-" sion from external causes, is from its structure among the least "mutable parts of the body." We shall see the Doctor in a little while comparing this same skin to bone. "And if," he observes a little farther, "the causes of colour have deeply "penetrated the texture of the skin, it becomes perpetual. Fi-"gures therefore, that are stained with paints inserted by punc-"tures made into its substance, can never be effaced."*

The great source of error in the Doctor's work is the use of the word skin. By skin, an anatomist means the cutis vera; but Dr. Smith uses it in three different senses: 1st, To signify rete mucosum; 2dly, The cutis; and, lastly, Both together. Now, there is no more resemblance between these two parts than there is between muscle and tendon. In one place he appears to understand its structure, and scarcely to have thought of it afterwards. I have before said, the colour of the body in health depended upon the rete mucósum solely, except in those who are fair. Those marks of which Dr. Smith speaks, are inserted in the cutis, and the colour is reflected through the rete. Their indelibility depends upon a cause of which Dr. Smith is entirely ignorant, and has quite mistaken. Extraneous bodies are removed by a system of vessels called absorbents. These can take up those things only which are soluble in our fluids; these colours not being so of necessity, remain.†

"Anatomists inform us," says the Doctor, "that like the "bones it," meaning the skin, "has few or no vessels, and there-"fore is not liable to those changes of augmentation or dimi-

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* Did the Doctor forget how quickly delicate people become tanned, as it is termed, upon exposure to a hot sun and dry air? Read the last part of the 9th and 10th page throughout, with the notes at the foot of the latter in Dr. Smith's Essay.

† Whether this reasoning be admitted or not, the fact is certain, that these colouring substances are inserted in the cutis.

"nution, and continual alteration of parts, to which the flesh, "the blood, and the whole vascular system is subject." Now, I am sure bone is the last part of the human body to which an anatomist would compare skin. In the next page, in a note likewise, "Anatomists know that people of colour have their "skins thicker than people of fair complexion in proportion to "the darkness of the hue." As the Doctor has not given us his authority for these two opinions, and as I never met with any thing like them, I conclude he quotes from memory, and, therefore, is liable to be mistaken. The former is certainly erroneous, and I believe the latter. One would have thought his own observation would have taught him better. Did he never observe that the skin bleeds upon being punctured with the finest instrument? So very vascular and sensible is it, as to appear to be almost a tissue of blood-vessels and nerves. The bile does not, as far as I know, tinge the rete muscosum, as Dr. Smith supposes, but remains in the cutis, and colours that in the same manner that it does the opaque cornea of the eye. Thus much for the Doctor's anatomy; now for his illustrations. "If," says he, but in a note, "the force of fire be "sufficient at a given distance to scorch the fuel, approach it as "much nearer as is proportional to the difference of heat be"tween our climate and that of Africa, and it will burn it black;" that is, as a sufficient degree of culinary heat will darken the colour of wood, so will a sufficient intensity of solar heat blacken the rete mucosum!

Were I to follow Dr. Smith through his whole work, you might suppose I have some enmity to that gentleman, which is certainly not the case; nor should I have mentioned him at all, had I not conceived it my duty. The execution does less credit to his head, than the intention to his heart.

That climate does produce great changes on all animals, and in man among others, no person will deny. Thus, if you transplant a sheep covered with wool from England into Syria, the wool will be changed into long silky kind of hair; but if you re-convey it to England, it will recover its wool.* Not so the African; he approximates not to the European by changing his climate, as is fully evinced on this continent. But what satisfies me that the colour does not depend upon climate and mode of life solely, is, that we find persons of such different complexions in the same latitude, and under nearly the same circumstances, and such similitude of appearance in the inhabitants of such extensive countries.

Ulloa informs us, that the unmixed descendants of the Spaniards at Guaquil, though inhabiting a warm country, within two degrees and a quarter of the line, are yet fresh coloured. The inhabitants of the deserts of Arabia are not at all like the Africans, though Bruce saw Fahrenheit's thermometer at 112° in the shade. Their country is sandy, and they are savage, and on these circumstances Dr. Smith lays great stress. On the contrary, the aboriginal inhabitants of this extensive continent were nearly of the same colour, the Esquimaux excepted. What is the reason that in all the latitudes and climates of the old world there were no red men? The inhabitants of high northern latitudes are tawny, cold producing the same effect as heat upon this hypothesis. But suppose all difficulty about the complexion and hair gotten over, can any man suppose that climate can affect the shape of the bones? Dr. Smith says indeed, that cold makes the heads of the Laplanders grow to an undue size, by contracting the face and the extremities—the head having more life.

I am afraid then the causes of these effects are unknown; and, as too frequently happens in physiological disquisitions, we are, for the present, obliged to confess our ignorance of them: that they were secondary, as I before said, there can be no doubt; perhaps, like the causes which have produced small-pox and syphilis, they may have ceased to operate. But on this subject, as on every other which may occur, I hold it my duty to lay before you all the facts which are relevant, all the opinions which are plausible, leaving you then to judge for yourselves. Different minds are satisfied with different degrees of evidence; and far be it from me to fix the bounds of your faith.

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* This is mentioned on the authority of Mr. Ashley Cooper, by whom the fact is stated in his lectures.

We shall in this as in other respects follow the same general plan which was pursued last year, except that the course now to be delivered, must necessarily be more extensive, as the Regents, since our last meeting, have done me the honour to appoint me to the Surgical as well as the Anatomical Chair. The treatment of burns and gun-shot wounds, not touched upon last winter, will now be laid down. We shall then, in the first place, demonstrate the structure of every part of the human body; next explain their functions as far as they are understood; and, lastly, the diseases and accidents to which they are liable, and the surgical treatment these may require.

For instance, the anatomical structure of the eye will first be demonstrated, then the manner in which its humours refract the rays of light so as to produce vision. The hypotheses will then be mentioned which have been brought forward to explain the way in which the eye adapts itself to the distinct perception of objects at different distances; and, lastly, I shall describe the diseases and accidents to which it is liable, and the remedies and operations they require. There are some accidents not peculiar to any part; with these then we shall conclude.

With regard to the opportunities which you will have of acquiring anatomical information, I am happy to inform you that we have made arrangements, owing to the liberality of the Legislature and of the gentlemen more particularly concerned, which will, I hope, prove satisfactory. But this is a subject on which I cannot now enter into detail. At our next meeting, gentlemen, the lecture will be on life, when I shall more at large lay down the plan to be pursued.

For the Medical and Philosophical Journal and Review.

A CASE *of* LUXATED HUMERUS *reduced after it had been displaced for* one hundred and twenty-*Days. Also a* CASE *in which a* LUXATION *of the* CLAVICLE *at its Scapular Articulation was successfully treated. By* JAMES COCKE, *M. D. of Baltimore.*

SOME time in the year 1805 I was requested by Dr. Potter to see a patient with him. The person was a Swedish sailor, who had embarked on board a brig bound to Baltimore from Bristol. He had been only a week at sea, when a flaw of wind gave a sudden and violent agitation to one of the sails, to which a rope that he held in his hand was attached. The motion of the sail was so unexpected and considerable, as to put it out of his power to disengage his hand, which was carried upwards with much force. The shoulder soon became very much pained; and in a short time was highly inflamed and swelled; with an inability of motion. There was no person on board of the vessel who could afford him relief, and he remained in great distress during the remaining part of the passage, which was not accomplished until eleven weeks after the accident. Immediately on his arrival in Baltimore he applied at the office of a physician, who happened to be at that time out of town. An apprentice of the physician undertook to manage the case, but from the swollen condition of the parts, or from some other cause, he did not discover that the head of the humerus was dislocated, and contented himself with making such local applications as he supposed would remove the swelling and inflammation. The patient was kept on that plan for some considerable time, when the physician returned and discovered the nature of the accident, which he attempted to relieve in the ordinary way, after having bled the man until he fainted; but from the force used being too small, or from so much of it being wasted on the muscles which attach the scapula to the body, the attempt did not succeed. When I saw the patient there was still some swelling about the shoulder, but the depression below the acromion was very evident. The head of the humerus, which had been thrown downwards, had made a socket for itself, in which it was so well accommodated, that the arm had become in some degree serviceable; the person was able to lift a chair from the floor, although the motion at the shoulder was very limited. The injured arm had lost its plumpness, and was much reduced in size. The pain about the shoulder had ceased, and being apprehensive of his sufferings being renewed, the person seemed disposed to continue in his crippled condition, rather than submit to another attempt being made to relieve him, unless we would give him almost positive assurances that he should not suffer in vain. However, after some hesitation and further delay, he gave himself up to our management. Discouraged as I was by a confession which I had heard made by Mr. Astly Cooper, of London, that he never had been able to reduce a luxated shoulder after eight weeks from the time of the accident, and knowing that Dr. Physic had acknowledged that he had never succeeded in a similar case after nine weeks, I undertook the reduction with much hesitation, and some little mortification at the man's having consented to a repetition of the experiment. I had seen some of the ablest surgeons foiled in their attempts to reduce luxated shoulders where the disorder had been of considerable duration, notwithstanding the aid of pullies, ropes, cushions, and other formidable apparatus. I had seen distracting force applied to a great extent; but from the difficulty of keeping the scapula in its natural situation, (which was attempted by cushioned bandages and other means) a very considerable part of the force was expended upon the muscles connecting the scapula to the body. To obviate this loss of force, and to retain the scapula in its place, appeared to me to be the principal points to be attained. For this purpose I rolled up a common coarse linen towel, and put it into the form of a ring of such a size as to admit the arm, and to pass easily over the shoulder. After fixing the ring on the shoulder, so that one part of it laid on the clavicle, and another part about the middle of the lower costa of the scapula, I attached one corner of a sheet to the fore part of the ring, and another to the back part of it, for the purpose of making counter-extension; in proportion to the force applied to the fore and back parts of the ring, it became narrower, and embraced the parts more closely. Napkins were fixed as usual on the injured arm. After these preparations were made, I desired the assistants who were there, on either side, to make a steady and forcible extension and counter extension; which they did without any other effect than that of giving the patient considerable pain, for the head of the bone remained immoveably fixed in the axilla. A vein was then opened, and the patient was directed to stand up as long as he could; the assistants were instructed to apply their force as soon as he fainted. After the loss of half a gallon of blood he fainted, and in his fall was placed on a small bench. The distraction was made at the proper time, and from his situation all muscular exertion was obviated; the head of the bone was without difficulty drawn out from the axilla, accompanied by a considerable noise of laceration. As soon as the head of the bone was sufficiently drawn out, the bone was easily returned to its natural socket, by suddenly depressing the elbow while my knee was placed in the axilla. The time that had elapsed from the accident to the reduction of the bone was about one hundred and twenty days. We then applied some bandages to prevent the part from becoming displaced.

The inflammation caused by the reduction very soon disappeared, and in a short time the arm began to have its power restored. When I saw the man last there remained from this accident only a slight stiffness of the thumb and fore-finger of the hand; but I understand from Dr. Potter, who saw him subsequently, that he was entirely freed from all the consequences of the accident, and that he entered on board of a ship as an able-bodied seaman.

I consider this case as valuable in several points of view. It shows that the advice of some surgeons who have recommended that after eight or ten weeks from the time of the accident no attempt should be made to relieve it, should not be regarded. It points out the utility of blood-letting in cases of this kind, which remedy has been revived and successfully practised by Dr. Physic; in as much as the same force was ineffectually applied before bleeding which succeeded after it.

I am also disposed to attach some importance to the ring which I used, and which appeared to me perfectly calculated to keep the scapula in its place, and to throw the distracting force on the only part where it could be useful.

Thomas Carroll, aged twenty-six years, in walking after night, attempted to pass over a pile of stones, from which he fell and struck the point of his shoulder. The accident gave him some pain on the instant, and he perceived that his arm was no longer capable of performing its ordinary motions. Two or three hours after the accident he called on me, and, on examination, I found the portion of the clavicle which was connected with the scapula displaced.

I had seen only two cases of the same kind before, both of which were in Guy's Hospital, and both of the patients left the institution without being relieved. A recollection of the failure in these cases had caused me to direct my attention considerably to the nature of the accident, and to the means which would probably be successful in relieving it. But I had not been able to satisfy myself that any plan of which I had thought would have the effect of keeping the shoulders sufficiently back, and of supporting the arm to a proper degree. I therefore gave a very doubtful prognostic as to the issue of the case. As the late hour of the night at which he called on me rendered it impracticable to have any machinery constructed for the occasion, I found myself under the necessity of resorting to the means within my reach.

After drawing the shoulder back until the clavicle was restored to its proper position on the scapula, I tied a cravat round the shoulder joint on either side, placing the knots about the middle of the base of the scapula, and leaving the ends of the cravat of considerable length. The arm was very well supported by a common string, made by a silk handkerchief, and the ends of the cravats were tied together and drawn so strongly as to bring both the shoulders back, and to retain them in the position which I wished; whereby the torn ends of the ligaments which connect the clavicle with the scapula were kept in apposition. I had occasion to adjust this simple contrivance every day for five or six days, by which time the parts appeared to have got into a very favourable condition, and in three or four weeks my patient returned to his trade, which was that of a smith, perfectly relieved.

To the Editors of the Medical and Philosophical Journal and Review.

GENTLEMEN,

The following case is sent to you for insertion on account of its singularity and importance. It was my intention to have subjoined some remarks, but upon writing to my friend, Mr. Baynham, of Virginia, who I knew had performed the operation in two instances with success, he was kind enough to promise me an account of them drawn up at length, with some observations on the symptoms which characterize the disease, and on the best manner of performing the operation. These will entirely supersede the necessity of my saying any thing on these topics. His communication will, I expect, be received in time for your next number, and will be immediately sent to the publishers.*

Yours, &c.
J. AUGUSTINE SMITH.

A CASE *of* EXTRA-UTERINE CONCEPTION, *in which an Operation was performed. By* J. AUGUSTINE SMITH.

ON Monday, the 10th of October, I was requested to visit a free negro woman, living in James-street, in consultation with several medical gentlemen of this city. The account we received from the patient was as follows: That she conceived, as she supposed, not quite twelve months from that time; that she felt the child move until she was seven months gone, according to her reckoning, after which all its motions ceased, at which time she was seized with what she supposed labour pains, which soon went off, and did not again recur: that she was conscious, during the whole course of her pregnancy, that the swelling was more on one side than on the other; she had menstruated regularly, and her breasts contained milk. The uterus, on examination, was found healthy. Ulceration had taken place in the tumour, she informed us, some weeks before; since which it had discharged a fetid matter, occasionally mixed with small pieces of bone. Upon examination we found a considerable enlargement, of which the umbilicus was nearly the centre, for it projected rather to the right side. There were two small orifices through which the matter made its way, the one somewhat above, the other directly in the umbilicus. Upon introducing a probe into the latter, bone could be distinctly felt at no great depth. As the patient was rapidly sinking under hectic fever and diarrhœa, it was unanimously decided that a speedy performance of an operation was the only means of saving her life, and the next morning was fixed upon for that purpose; the patient having consented, after being apprised of its danger and uncertainty. Accordingly the next morning I performed the operation. Having first introduced a probe into the sac, I made an incision at the inner edge of the right rectus abdominis muscle, from an inch and a half above, to the same distance below the umbilicus. At three strokes of the knife I got into the sac, and introducing my finger, dilated upwards and downwards as far as could be done with safety; for had I gone beyond the adhesion of the sac to the parieties of the abdomen, and thus exposed the cavity of the peritoneum, the patient must have died. I then proceeded to extract the bones with my dressing forceps, conducted by my finger, which at first was easily done, but after removing two or three it became more and more difficult, and at last I found it absolutely impracticable to get away any more, at least without doing a violence to the woman which she could not possibly survive. This disappointment arose from a circumstance which it was impossible to foresee; for, from the length of time which had elapsed from the death of the fœtus and other circumstances, we had every reason to suppose that all the soft parts of it were destroyed by putrefaction; but as this unfortunately had not happened, I was obliged to desist. This I did the more willingly, as I was convinced if the patient did not die of symptomatic fever, that the putrefactive process would now go on much more rapidly, and that the bones might be taken away as they became detached. The event justified my expectation. The wound was filled with lint, and the patient put to bed. She lost an inconsiderable quantity of blood during the operation.

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* This operation is so extremely rare, that we have been unable to find any instance of it on record except the first which occurred to Mr. Baynham, and of this there is a mere sketch; we shall, therefore, feel particularly obliged to this gentleman for his communications on the subject. *Editors.*

Five o'clock P. M. She complains of great pain, and has puked several times; but to this she has been liable every day for some time. No pain upon pressing any part of the abdomen except near the wound. A gentle cathartic ordered.

12th. The wound, which looked foul and sloughy, had discharged a little; a poultice was directed to be applied to it, and the cathartic to be repeated, the other not having operated. The fever not very high.

13th. Much better, the cathartic had operated twice. The appearance of the wound very much improved. On wiping it, two pieces of bone were discharged. The tumour considerably decreased in size.

14th. Passed a bad night in consequence of pain.

15th. More nourishing diet and a few drops of diluted sulphuric acid ordered. The discharge so great that the patient thought some fluid which she drank had passed through the wound; it is not very fetid. An opiate ordered at bed time, as she complained of great pain, and want of rest.

15th. This day I removed the trunk of the fœtus entire, the wound was dressed with prest sponge; she was directed to take bark in the day and opium at night.

After this her health rapidly improved, and all the remaining bones of the skeleton were ultimately extracted, those belonging to the head excepted; whether there were any or not is uncertain, for none have ever been felt, and the cavity filled with granulations from the bottom; they may perhaps be inclosed in a cyst, or they may hereafter be felt and extracted. The woman is now as fat and as healthy as she ever was.

From the size of the bones I should judge the fœtus to have been about four months old; but it might have lived seven, and its diminutive size be the consequence of deficient nutrition, as from its unnatural situation it was probably not so well supplied with blood as it would have been in the uterus.

The importance of the discovery announced in the last celebrated Bakerian Lecture of Mr. Davy, of the Royal Institution of London, and the interesting nature of the experiments detailed in that performance, have induced us to gratify our readers by inserting the whole of it. A few copies only can have been received from Europe, and we do not know that it has yet been reprinted in the United States, so that it is very scarce and not easily procured. For this reason the entire Lecture will be more acceptable than any abstract or abridgement we could possibly give. In connection with this we shall further endeavour to insert, under the article of Intelligence, *a summary of the contents of the Bakerian Lecture of the preceding year, on other chemical phenomena of galvanic electricity.*

EDITORS.

The BAKERIAN LECTURE *on some new Phenomena of Chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalies, and the Exhibition of the new Substances which constitute their Bases; and on the general Nature of Alkaline Bodies. By* HUMPHRY DAVY, *Esq. Secretary to the Royal Society.*

I. *Introduction.*

IN the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a number of decompositions and chemical changes produced in substances of known composition by electricity; and I ventured to conclude, from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electro-chemical analysis, to bodies which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results.

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections; particularly those which demonstrate the decomposition and composition of the fixed alkalies, and the production of the new and extraordinary bodies which constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed, and often repeated.

II. *On the Methods used for the Decomposition of the fixed Alkalies.*

The researches I had made on the decomposition of acids, and of alkaline and earthly neutral compounds, proved that the powers of electrical decomposition were proportional to the strength of the opposite electricities in the circuit, and to the conducting power and degree of concentration of the materials employed.

In the first attempts that I made on the decomposition of the fixed alkalies, I acted upon aqueous solutions of potash and soda, saturated at common temperatures, by the highest electrical power I could command, and which was produced by a combination of voltaic batteries, belonging to the Royal Institution, containing twenty-four plates of copper and zinc of twelve inches square, one hundred plates of six inches, and one hundred and fifty of four inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hydrogen and oxygen disengaged with the production of much heat and violent effervescence.—The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxygen gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containing potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery, of the power of one hundred of six inches, highly charged; and the connection from the negative side was made by a platina wire. By this arrangement some brilliant phenomena were produced. The potash appeared a conductor, in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the developement of combustible matter, arose from the point of contact. When the order was changed, so that the platina spoon was made negative, a vivid and constant light appeared at the opposite point. There was no effect of inflammation round it; but aeriform globules, which inflamed in the atmosphere, rose through the potash.

The platina, as might have been expected, was considerably acted upon; and in the cases when it had been negative in the highest degree.

The alkali was apparently dry in this experiment; and it seemed probable, that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained, indeed, a number of dark grey metallic particles, but these proved to be derived from the platina.

I tried several experiments on the electrization of potash, rendered fluid by heat, with the hopes of being able to collect the combustible matter, but without success; and I only attained my object, by employing electricity, as the common agent for fusion and decomposition. Though potash, perfectly dried by ignition, is a non-conductor, yet it is rendered a conductor by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery, of the power of two hundred and fifty of six and four, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Under these circumstances, a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface: at the lower or negative surface, there was no liberation of elastic fluid; but small globules, having a high metallic lustre, and being precisely similar, in visible characters to quicksilver, appeared; some of which burnt with explosion, and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film, which formed on their surfaces.

These globules, numerous experiments soon showed to be the substance I was in search of, and a peculiar inflammable principle the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and a substance of the same kind was produced, when pieces of copper, silver, gold, plumbage, and even charcoal were employed for completing the circuit.

The phenomenon was independent of the presence of air. I found that it took place when the alkali was in the vacuum of an exhausted receiver.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires, by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of the alkali, and this substance soon penetrated through the body of the tube.

Soda, when acted upon in the same manner as potash, exhibited an analogous result; but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of one hundred of six inches in full activity, I obtained good results from pieces of potash weighing from forty to seventy grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than fifteen and twenty grains in weight, and that only when the distance between the wires was about one eighth or one tenth of an inch.

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared having the lustre of silver.

When the power of two hundred and fifty was used with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air, in a state of vivid combustion, producing a beautiful effect of continued jets of fire.

III. *Theory of the Decomposition of the fixed Alkalies; their Composition and Production.*

As in all decompositions of compound substances which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxygen was produced, and evolved or carried into combination at the positive surface; it was reasonable to conclude that this substance was generated in a similar manner by the electrical action upon the alkalies, and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case.

When solid potash, or soda in its conducting state, was included in glass tubes, furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved, by the most delicate examination, to be pure oxygen; and unless an excess of water was present, no gas was evolved from the negative surface.

In the synthetical experiments, a perfect coincidence likewise will be found.

I mentioned that the metallic lustre of the substance from potash immediataly became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere, till the whole globule disappeared, and assumed the form of a saturated solution of potash.*

When globules were placed in appropriate tubes, containing common air or oxygen gas, confined by mercury, an absorption of oxygen took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its solution the process stopped, the interior being defended from the action of the gas.

With the substances from soda the appearances and effects were analogous. When the substances were strongly heated, confined in given portions of oxygen, a rapid combustion with a brilliant white flame was produced, and the metallic globules were found converted into a white and solid mass, which, in the case of the substance from potash, was found to be potash, and in the case of that from soda, soda.

Oxygen gas was absorbed in this operation, and nothing emitted which effected the purity of the residual air. The alkalies produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxygen gas absorbed; and their weights considerably exceeded those of the combustible matters consumed. The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxygen and of the respective inflammable substances which enter into union to form the fixed alkalies will be given.

It appears, then, that in these facts there is the same evidence for the decomposition of potash and soda into oxygen and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxyds into oxygen and their respective combustible bases.

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* Water likewise is decomposed in the process. We shall hereafter see that the bases of the fixed alkalies act upon this substance with greater energy than any other known bodies. The minute theory of the oxydation of the bases of the alkalies in the free air is this: oxygen gas is first attracted by them, and alkali formed. This alkali speedily absorbs water. This water is again decomposed. Hence, during the conversion of a globule into alkaline solution, there is a constant and rapid disengagement of small quantities of gas.

In the analytical experiments, no substances capable of decomposition are present, but the alkalies and a minute portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali; they come in contact with the heated moistened surface; they cannot be produced from crystallized alkalies, which contain much water; and the effects produced by the electrization of ignited potash, which contains no sensible quantity of water, confirm the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalies seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces; and the oxygen follows the contrary order;* or the oxygen being naturally possessed of the negative energy, and the bases of the positive do not remain in combination when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid union is the result, as in other like cases with the production of fire.

A number of circumstances relating to the agencies of the bases will be immediately stated, and will be found to offer confirmations to these general conclusions.

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* See Bakerian Lecture, 1806, page 28, Philosophical Transactions for 1807.

IV. *On the Properties and Nature of the Basis of Potash.*

After I had detected the bases of the fixed alkalies, I had considerable difficulty to preserve and confine them so as to examine their properties, and submit them to experiments; for, like the alkahests imagined by the alchemists, they acted more or less upon almost every body to which they were exposed.

The fluid substance amongst all those I have tried, on which I find they have least effect, is recently distilled naphtha. In this material, when excluded from the air, they remain for many days without considerably changing, and their physical properties may be easily examined in the atmosphere when they are covered by a thin film of it. The basis of potash at 60° Fahrenheit, the temperature in which I first examined it, appeared, as I have already mentioned, in small globules, possessing the metallic lustre, opacity and general appearance of mercury; so that when a globule of mercury was placed near a globule of the peculiar substance, it was not possible to detect a difference by the eye.

At 60° Fahrenheit it is, however, only imperfectly fluid, for it does readily run into a globule when its shape is altered; at 70° it becomes more fluid; and at 100° its fluidity is perfect, so that different globules may be easily made to run into one. At 50° Fahrenheit it becomes a soft and malleable solid, which has the lustre of polished silver; and at about the freezing point of water it becomes harder and brittle; and, when broken in fragments, exhibits a crystallized texture, which, in the microscope, seems composed of beautiful facets of a perfect whiteness and high metallic splendour.

To be converted into vapour, it requires a temperature approaching that of the red heat; and when the experiment is conducted under proper circumstances, it is found unaltered after distillation.

It is a perfect conductor of electricity. When a spark from the voltaic battery of an hundred of six inches is taken upon a large globule in the atmosphere, the light is green, and combustion takes place at the point of contact only. When a small globule is used it is completely dissipated with explosion, accompanied by a most vivid flame, into alkaline fumes. It is an excellent conductor of heat. Resembling the metals in all these sensible properties, it is, however, remarkably different from any of them in specific gravity. I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was eight hundred and sixty-one, and it did not sink in double distilled naphtha, the specific gravity of which was about seven hundred and seventy, that of water being considered as one. The small quantities in which it is produced by the highest electrical powers, rendered it very difficult to determine this quality with minute precision. I endeavoured to gain approximations on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury. I used the very delicate balance of the Royal Institution, which, when loaded with the quantities I employed, and of which the mercury never exceeded ten grains, is sensible, at least, to the $\frac{1}{1000}$ of a grain. Taking the mean of four experiments, conducted with great care, its specific gravity at 62° Fahrenheit, is to that of mercury as 10 to 223, which gives a proportion to that of water nearly as 6 to 10; so that it is the lightest fluid body known. In its solid form it is a little heavier; but even in this state, when cooled to 40° Fahrenheit, it swims in the double distilled naphtha.

The chemical relations of the basis of potash are still more extraordinary than its physical ones.

I have already mentioned its alkalization and combustion in oxygen gas. It combines with oxygen slowly and without flame at all temperatures that I have tried below that of its evaporation. But at this temperature combustion takes place, and the light is of a brilliant whiteness, and the heat intense. When heated slowly in a quantity of gas not sufficient for its complete conversion into potash, and at a temperature inadequate to its inflammation, 400° Fahrenheit for instance, its tint changes to that of a red brown, and when the heat is withdrawn, all the oxygen is found to be absorbed, and a solid is formed of a greyish colour, which partly consists of potash, and partly of the basis of potash in a lower degree of oxygenation, and which becomes potash by being exposed to water, or by being again heated in fresh quantities of air. The substance consisting of the basis of potash combined with an under proportion of oxygen, may likewise be formed by fusing dry potash and its basis together under proper circumstances. The basis rapidly loses its metallic splendour; the two substances unite into a compound of a red brown colour when fluid, and of a dark grey hue when solid; and this compound soon absorbs its full proportion of oxygen when exposed to the air, and is wholly converted into potash.

And the same body is often formed in the analytical experiments when the action of the electricity is intense, and the potash much heated.

The basis of potash, when introduced into oxymuriatic acid gas, burns spontaneously with a bright red light, and a white salt, proving to be muriate of potash, is formed.

When a globule is heated in hydrogen at a degree below its point of vaporization, it seems to dissolve in it, for the globule diminishes in volume, and the gas explodes with alkaline fumes and bright light, when suffered to pass into the air; but by cooling, this spontaneous detonating property is destroyed, and the basis is either wholly or principally deposited.

The action of the basis of potash on water exposed to the atmosphere is connected with some beautiful phenomena. When it is thrown upon water, or when it is brought into contact with a drop of water at common temperatures, it decomposes it with great violence, an instantaneous explosion is produced with brilliant flame, and a solution of pure potash is the result.

In experiments of this kind, an appearance often occurs similar to that produced by the combustion of phosphorated hydrogen; a white ring of smoke, which gradually extends as it rises into the air.

When water is made to act upon the basis of potash out of the contact of air, and preserved by means of a glass tube under naphtha, the decomposition is violent; and there is much heat and noise but no luminous appearance; and the gas evolved, when examined in the mercurial or water pneumatic apparatus, is found to be pure hydrogen.

When a globule of the basis of potash is placed upon ice, it instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash.

The theory of the action of the basis of potash upon water exposed to the atmosphere, though complicated changes occur, is far from being obscure. The phenomena seem to depend on the strong attractions of the basis for oxygen and of the potash formed for water. The heat, which arises from two causes, decomposition and combination, is sufficiently intense to produce the inflammation. Water is a bad conductor of heat; the globule seems exposed to air; a part of it, there is the greatest reason to believe, is dissolved by the heated nascent hydrogen; and this substance being capable of spontaneous inflammation, explodes and communicates the effect of combustion to any of the basis that may be yet uncombined.

When a globule confined out of the contact of air is acted upon by water, the theory of decomposition is very simple; the heat produced is rapidly carried off, so that there is no ignition; and a high temperature being requisite for the solution of the basis in hydrogen, this combination probably does not take place, or at least it may have a momentary existence only.

The production of alkali in the decomposition of water by the basis of potash, is demonstrated in a very simple and satisfactory manner by dropping a globule of it upon moistened paper tinged with termeric. At the moment that the globule comes into contact with the water, it burns, and moves rapidly upon the paper, as if in search of moisture, leaving behind it a deep reddish-brown trace, and acting upon the paper precisely as dry caustic potash.

So strong is the attraction of the basis of potash for oxygen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified.

In ether this decomposition is connected with an instructive result. Potash is insoluble in this fluid; and when the basis of potash is thrown into it, oxygen is furnished to it, and hydrogen disengaged, and the alkali, as it forms, renders the ether white and turbid.

In both these inflammable compounds the energy of its action is proportionable to the quantity of water they contain, and hydrogen and potash are the constant result.

The basis of potash, when thrown into solutions of the mineral acids, inflames and burns on the surface. When it is plunged by proper means beneath the surface enveloped in potash, surrounded by naphtha, it acts upon the oxygen with the greatest intensity, and all its effects are such as may be explained from its strong affinity for this substance. In sulphuric acid a white saline substance, with a yellow coating, which is, probably, sulphate of potash surrounded by sulphur, and a gas which has the smell of sulphureous acid, and which, probably, is a mixture of that substance with hydrogen gas, are formed. In nitrous acid, nitrous gas is disengaged, and nitrate of potash formed.

The basis of potash readily combines with the simple inflammable solids, and with the metals; with phosphorus and sulphur it forms compounds similar to the metallic phosphorets and sulphurets. When it is brought in contact with a piece of phosphorus and pressed upon, there is a considerable action: they become fluid together, burn, and produce phosphorate of potash. When the experiment is made under naphtha, their combination takes place without the liberation of any elastic matter, and they form a compound which has a considerably higher point of fusion than its two constituents, and which remains a soft solid in boiling naphtha. In its appearance it perfectly agrees with a metallic phosphoret; it is of the colour of lead, and, when spread out, has a lustre similar to polished lead. When exposed to air at common temperatures it slowly combines with oxygen, and becomes phosphate of potash. When heated upon a plate of platina, fumes exhale from it, and it does not burn till it attains the temperature of the rapid combustion of the basis of potash. When the basis of potash is brought in contact with sulphur in fusion, in tubes filled with the vapour of naphtha, they combine rapidly with the evolution of heat and light, and a grey substance, in appearance like artificial sulphuret of iron, is formed, which, if kept in fusion, rapidly dissolves the glass, and becomes bright brown. When this experiment is made in a glass tube hermetically sealed, no gas is liberated if the tube is opened under mercury; but when it is made in a tube connected with a mercurial apparatus, a small quantity of sulphureted hydrogen is evolved, so that the phenomena are similar to those produced by the union of sulphur with the metals in which sulphureted hydrogen is likewise disengaged, except that the ignition is stronger.*

Copper filings and powdered sulphur, in weight in the proportion of three to one, rendered very dry, were heated together in a retort, connected with a mercurial pneumatic apparatus. At the moment of combination a quantity of elastic fluid was liberated, amounting to nine or ten times the volume of materials employed, and which consisted of sulphureted hydrogen mixed with sulphureous acid. The first mentioned product, there is every reason to believe, must be referred to the sulphur; the last probably to the copper, which, it is easy to conceive, may have become slightly and superficially oxydated during the processes of filing and drying by heat.

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* The existence of hydrogen in sulphur, is rendered very probable by the ingenious researches of M. Berthollet, jun. Annales de Chemie, Fevrier 1807. p. 143. The fact is almost demonstrated by an experiment which I saw made by W. Hayfield, Esq. of Bristol, in 1799.

When the union is effected in the atmosphere, a great inflammation takes place, and sulphuret of potash is formed. The sulphureted basis likewise gradually becomes oxygenated by exposure to the air, and is finally converted into sulphate. The new substance produces some extraordinary and beautiful results with mercury. When one part of it is added to eight or ten parts of mercury in volume at 60° Fahrenheit, they instantly unite and form a substance exactly like mercury in colour, but which seems to have less coherence; for small portions of it appear as flattened spheres. When a globule is made to touch a globule of mercury about twice as large, they combine with considerable heat; the compound is fluid at the temperature of its formation; but, when cool, it appears as a solid metal, similar in colour to silver. If the quantity of the basis of potash is still farther increased, so as to be about one thirtieth the weight of the mercury, the amalgam increases in hardness, and becomes more brittle. The solid amalgam, in which the basis is in the smallest proportion, seems to consist of about one part in weight of basis, and seventy parts of mercury, and is very soft and malleable.

When these compounds are exposed to air, they rapidly absorb oxygen; potash, which deliquesces, is formed, and in a few minutes the mercury is found pure and unaltered.

When a globule of the amalgam is thrown into water it rapidly decomposes it, with a hissing noise; potash is formed, pure hydrogen is disengaged, and the mercury remains free.

The fluid amalgam of mercury and this substance dissolves all the metals I have exposed to it; and in this state of union mercury acts on iron and platina. When the basis of potash is heated with gold, or silver, or copper, in a close vessel of pure glass, it rapidly acts upon them; and when the compounds are thrown into water, this fluid is decomposed, potash formed, and the metals appear to be separated unaltered.

The basis of potash combines with fusible metal, and forms an alloy with it, which has a higher point of fusion than the fusible metal.

The action of the basis of potash upon the inflammable oily compound bodies, confirms the other facts of the strength of its attraction for oxygen.

On naphtha, colourless and recently distilled, as I have already said, it has very little power of action; but in naphtha that has been exposed to the air, it soon oxydates, and alkali is formed, which unites with the naphtha into a brown soap that collects round the globule.

On the concrete oils, (tallow, spermaceti, wax, for instance) when heated it acts slowly, coaly matter is deposited, a little gas* is evolved, and a soap is formed; but in these cases it is necessary that a large quantity of the oil be employed. On the fluid fixed oils it produces the same effects, but more slowly.

By heat likewise it rapidly decomposes the volatile oils; alkali is formed, a small quantity of gas is evolved, and charcoal is deposited.

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* When a globule of the basis of potash is introduced into any of the fixed oils heated, the first product is pure hydrogen, which arises from the decomposition of the water absorbed by the crust of potash during the exposure to the atmosphere. The gas evolved when the globule is freed from the crust, I have found to be carbonated hydrogen, requiring more than an equal bulk of oxygen gas for its complete saturation by explosion. I have made a great number of experiments which it would be foreign to the object of this lecture to give in minute detail, on the agencies of the basis of potash on the oils. Some anomalies occurred which led to the inquiry, and the result was perfectly conclusive. Olive oil, oil of turpentine, and naphtha, when decomposed by heat, exhibited, as products, different proportions of charcoal, heavy inflammable gas, empyreumatic oily matter, and water, so that the existence of oxygen in them was fully proved; an accurate indication of the proportions of their element might be gained by their decomposition by the basis of potash. Naphtha of all furnished least water, and carbonic acid and oil of turpentine the most.

When the basis of potash is thrown into camphor in fusion, the camphor soon becomes blackened, no gas is liberated in the process of decomposition, and a saponaceous compound is formed; which seems to show that camphor contains no more oxygen than the volatile oils.

The basis of potash readily reduces metallic oxyds when heated in contact with them. When a small quantity of the oxyd of iron was heated with it, to a temperature approaching its point of distillation, there was a vivid action; alkali and grey metallic particles, which dissolved with effervescence in muriatic acid, appeared.

The oxyds of lead and the oxyds of tin were revived still more rapidly; and when the basis of potash was in excess, an alloy was formed with the revived metal.

In consequence of this property the basis of potash readily decomposes flint glass, and green glass, by a gentle heat; alkali is immediately formed by oxygen from the oxyds, which dissolves the glass, and a new surface is soon exposed to the agent. At a red heat even the purest glass is altered by the basis of potash: the oxygen in the alkali of the glass seems to be divided between the two bases, the basis of potash and the alkaline basis in the glass and oxyds, in the first degree of oxygenation, are the result. When the basis of potash is heated in tubes made of plate glass, filled with vapour of naphtha, it first acts upon the small quantity of oxyds of cobalt and manganese in the interior surface of the glass, and a portion of alkali is formed. As the heat approaches to redness it begins to rise in vapour, and condense in the colder parts of the tube; but at the point, where the heat is strongest, a part of the vapour seems to penetrate the glass, rendering it of a deep red-brown colour; and by repeatedly distilling and heating the substance in a close tube of this kind, it finally loses its metallic form, and a thick brown crust, which slowly decomposes water, and which combines with oxygen when exposed to air, forming alkali, lines the interior of the tube, and in many parts is found penetrating through its substance.*

In my first experiments on the distillation of the basis of potash I had great difficulty in accounting for these phenomena; but the knowledge of the substance it forms in its first degree of union with oxygen afforded a satisfactory explanation.

V. *On the Properties and Nature of the Basis of Soda.*

The basis of soda, as I have already mentioned, is a solid at common temperatures. It is white, opaque, and when examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is much softer than any of the common metallic substances. When pressed upon by a platina blade with a small force, it spreads into thin leaves, and a globule of the one-tenth or one-twelfth of an inch in diameter is easily spread over a surface of a quarter of an inch,† and this property does not diminish when it is cooled to 32° Fahrenheit. It conducts electricity and heat in a similar manner to the basis of potash, and small globules of it inflame by the voltaic electrical spark, and burns with bright explosions.

Its specific gravity is less than that of water. It swims in oil of sassafras of 1.096, water being one, and sinks in naphtha of specific gravity .861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions till I had composed a fluid in which it remained at rest above or below; and this fluid consisted of nearly twelve parts of naphtha and five of oil of sassafras, which gives a specific gravity to that of water nearly as 9 to 10, or, more accurately, as .9348 to 1.

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* This is the more obvious explanation in the present state of our knowledge; but it is more than probable that the silex of the glass likewise suffers some change, and probably decomposition. The subject I hope to be able to resume on another occasion.

† Globules may be easily made to adhere and form one mass by strong pressure, so that the property of welding, which belongs to iron and platina at a white heat only, is possessed by this substance at common temperatures.

The basis of soda has a much higher point of fusion than the basis of potash; its parts begin to lose their cohesion at about 120° Fahrenheit; and it is a perfect fluid at about 180°; so that it readily fuses under boiling naphtha.

I have not yet been able to ascertain at what degree of heat it is volatile, but it remains fixed in a state of ignition at the point of fusion of plate glass.

The chemical phenomena produced by the basis of soda are analogous to those produced by the basis of potash, but with such characteristic differences as might be well expected.

When the basis of soda is exposed to the atmosphere, it immediately tarnishes, and, by degrees, becomes covered with a white crust, which deliquesces much more slowly than the substance which forms on the basis of potash. It proves, on minute examination, to be pure soda.

The basis of soda combines with oxygen slowly, and without luminous appearance, at all common temperatures; and, when heated, this combination becomes more rapid; but no light is emitted till it has acquired a temperature nearly that of ignition.

The flame that it produced in oxygen gas is white, and it sends forth bright sparks, occasioning a very beautiful effect; in common air it burns with light of the colour of that produced during the combustion of charcoal, but much brighter.

The basis of soda, when heated in hydrogen, seems to have no action upon it. When introduced into oxymuriatic acid gas it burns vividly, with numerous scintillations of a bright red colour. Saline matter was formed in this combustion, which, as might have been expected, proved to be muriate of soda. Its operation on water offers most satisfactory evidence of its nature. When thrown upon this fluid, it produces a violent effervescence, with a loud hissing noise; it combines with the oxygen of the water to form soda, which is dissolved, and its hydrogen is disengaged. In this operation there is no luminous appearance, and it seems probable that even in the nascent state hydrogen is incapable of combining with it.*

When the basis of soda is thrown into hot water the decomposition is more violent, and in this case a few scintillations are generally observed at the surface of the fluid; but this is owing to small particles of the basis which are thrown out of the water sufficiently heated to burn in passing through the atmosphere. When, however, a globule is brought in contact with a small particle of water, or with moistened paper, the heat produced (there being no medium to carry it rapidly) is usually sufficient for accension of the basis.

The basis of soda acts upon alkohol and ether precisely in a similar manner with the basis of potash. The water that they contain is decomposed, soda is rapidly formed, and hydrogen disengaged.

The basis of soda, when thrown upon the strong fluids, acts with great energy. When nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acid there is much heat generated, but no light.

When plunged by proper means beneath the surface of the acids, it is rapidly oxygenated; soda is produced, and the other products are similar to those generated by the action of the basis of potash. With respect to the fixed and volatile oils and naphtha in their different states, there is a perfect coincidence between the effects of the two new substances, except in the difference of the appearances of the saponaceous compounds formed; those produced by the oxydation and combination of the basis of soda being of a darker colour, and apparently less soluble.

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* The more volatile metals seem capable of uniting with hydrogen, a circumstance presenting an analogy.

The basis of soda, in its degrees of oxydation, has precisely similar habits with the basis of potash.

When it is fused with dry soda in certain quantities, there is a division of oxygen between the alkali and the base, and a deep brown fluid is produced, which becomes a dark grey solid on cooling, and which attracts oxygen from the air, or which decomposes water and becomes soda.

The same body is often formed in the analytical processes of decomposition; and it is generated when the basis of soda is fused in tubes of the purest plate glass.

There is scarcely any difference in the visible phenomena of the agencies of the basis of soda and that of potash on sulphur, phosphorus and the metals. It combines with sulphur in close vessels filled with the vapour of naphtha with great vividness, with light, heat, and often with explosion from the vaporization of a portion of the sulphur, and the disengagement of sulphureted hydrogen gas. The sulphureted basis of soda is of a deep grey colour.

The phospheret has the appearance of lead, and forms phosphate of soda by exposure to air or by combustion. The basis of soda, in the quantity of one-fortieth, renders mercury a fixed solid, of the colour of silver, and the combination is attended with a considerable degree of heat.

It makes an alloy with tin without changing its colours, and it acts upon lead and gold when heated. I have not examined its habitudes with any other metals; but in its state of alloy it is soon converted into soda by exposure to air, or by the action of water, which it decomposes with the evolution of hydrogen.

The amalgam of mercury and the basis of soda seem to form triple compounds with other metals. I have tried iron and platina, which, I am inclined to believe, remain in combination with the mercury when it is deprived of the new substance by exposure to air. The amalgam of the basis of soda and mercury likewise combines with sulphur, and forms a triple compound of a dark grey colour.

VI. *On the Proportions of the peculiar Basis and Oxygen in Potash and Soda.*

The facility of combustion of the bases of the alkalies, and the readiness with which they decompound water, offered means fully adequate for determining the proportions of their ponderable constituent parts. I shall mention the general methods of the experiments, and the results obtained by the different series, which approach as near to each other as can be expected in operations performed on such small quantities of materials.

For the process in oxygen gas I employed glass tubes containing small trays made of thin leaves of silver or other noble metals, on which the substance to be burned, after being accurately weighed or compared with a globule of mercury equal in size,* was placed: the tube was small at one end, curved, and brought to a fine point, but suffered to remain open; and the other end was fitted to a tube communicating with a gasometer, from which the oxygen gas was introduced, for neither water nor mercury could be used for filling the apparatus. The oxygen gas was carried through the tube till it was found that the whole of the common air was expelled. The degree of its purity was acertained by suffering a similar quantity to pass into the mercurial apparatus. The lower orifice was then hermetically sealed by a spirit-lamp, and the upper part drawn out and finally closed, when the aperture was so small as to render the temperature employed incapable of materially influencing the volume of the gas; and when the whole arrangement was made, the combination was effected by applying heat to the glass, in contact with the metallic tray.

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* When the globules were very small the comparison with mercury, which may be quickly made by means of a micrometer, was generally employed as the means of ascertaining the weight; for in this case the globule would be immediately introduced into the tube, and the weight of the mercury ascertained at leisure.

In performing these experiments many difficulties occurred. When the flame of the lamp was immediately brought to play upon the glass, the combustion was very vivid, so as sometimes to break the tube, and the alkali generated partly rose in white fumes, which were deposited upon the glass.

When the temperature was slowly raised, the basis of the alkalies acted upon the metallic trays and formed alloys; and in this state it was very difficult to combine them with their full proportion of oxygen; and glass alone could not be employed on account of its decomposition by the alkaline basis; and porcelain is so bad a conductor of heat, that it was not possible to raise it to the point required for the process without softening the glass.

In all cases the globules of the alkaline basis were carefully freed from naphtha before they were introduced; of course a slight crust of alkali was formed before the combustion; but this could not materially affect the results; and when such a precaution was not used, an explosion generally took place from the vaporization and decomposition of the film of naphtha surrounding the globule.

After the combustion the absorption of gas was acertained by opening the lower point of the tube under water or mercury. In some cases the purity of the residual air was ascertained; in others the alkali formed in the tray was weighed.

From several experiments on the synthesis of potash by combustion, I shall select two which were made with every possible attention to accuracy, and under favourable circumstances for a mean result.

In the first experiment 0.12 grains of the basis of soda were employed. The combustion was made upon platina, and was rapid and complete; and the basis appeared to be perfectly saturated, as no disengagement of hydrogen took place when the platina tray was thrown into the water. The oxygen gas absorbed equalled in volume 190 grains of quicksilver, barometer being at 29.8 inches, thermometer 62° Fahrenheit, and this reduced to a temperature of 60° Fahrenheit, and under a pressure equal to that indicated by 30 inches* would become 186.67 measures; the weight of which would be about .0184 grains troy,† but .0184 : .1384 : : 13. 29 : 100; and, according to this estimation, 100 parts of potash will consist of 86.7 basis, and 13.3 oxygen nearly.

In the second experiment .07 grains of the basis absorbed at temperature 63° of Fahrenheit, and under pressure equal to 30.1 barometer inches, a quantity of oxygen equal in volume to 121 grains of mercury; and the proper corrections being made, as in the former case, this gas would weigh .01189 grains.

But as .07+01189=.08.189 : 07 : : 100 : 85.48 nearly, and 100 parts of potash will consist of 85.5 of basis, and 14.5 of oxygen nearly; and the mean of the two experiments will be 86.1 of basis to 13.9 of oxygen for 100 parts.

In the most accurate experiment that I made on the combustion of the basis of soda, .08 parts of the basis absorded a quantity of oxygen equal in volume to 206 grains of mercury; the thermometer being at 56° Fahrenheit, and the barometer at 29.4; and this quantity, the correction being made as before for the mean temperature and pressure, equals about .02 grains of oxygen.

And as .08+02=.10 : .08 : : 100 : 80, and 100 parts of soda, according to this estimation, will consist of 80 basis to 20 oxygen.

In all cases of slow combustion, in which the alkalies were not carried out of the tray, I found a considerable increase of weight; but as it was not possible to weigh them except in the atmosphere, the moisture attracted rendered the result doubtful, and the proportions from the weight of the oxygen absorbed are more to be depended on. In the experiments in which the processes of wieghing were most speedily performed, and in which no alkali adhered to the tube, the basis of potash gained nearly two parts for ten, and that of the soda between three and four parts.

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* In the correction for temperature the estimations of Dalton and Gay-Lussac are taken, which make gasses expand about $\frac{1}{480}$ of the primitive volume for every degree of Fahrenheit.

† From experiments which I made in 1799 on the specific gravity of oxygen gas, it would appear that its weight is to that of water as 1 to 718, and to that of quicksilver as 1 to 10142. Researches Chem. and Phil. p. 9, and with this estimation, that deducible from the late accurate researches of Messrs. Allen and Pepys on the combustion of the diamond, almost precisely agrees. Phil. Trans. 1807, p. 275.

The results of the decompositions of water by the bases of the alkalies, were much more readily and perfectly obtained than those of their combustion.

To check the rapidity of the process, and in the case of potash to prevent any of the basis from being dissolved, I employed the amalgams with mercury. I used a known weight of the bases, and made the amalgams under naphtha, using about two parts of mercury in volume to one of basis.

In the first instances I placed the amalgams under tubes filled with naphtha, and inverted in glasses of naphtha, and slowly admitted water to the amalgam at the bottom of the glass; but this precaution I soon found unnecessary, for the action of the water was not so intense but that the hydrogen gas could be wholly collected.

I shall give an account of the most accurate experiments made on the decomposition of water by the bases of potash and soda.

In an experiment on the basis of potash, conducted with every attention that I could pay to the minutiæ of the operation, hydrogen gas equal in volume to 298 grains of mercury, was disengaged by the action of .08 grains of the basis of potash, which had been amalgamated with about three grains of mercury. The thermometer at the end of the process indicated a temperature of 56° Fahrenheit, and the barometer an atmospheric pressure equal to 29.6 inches.

Now, this quantity of hydrogen* would require for its combustion a volume of oxygen gas about equal to that occupied by 154.9 grains of mercury, which gives the weight of oxygen to saturate the .08 grains of the basis of potash at the mean temperature and pressure, nearly .0151 grains; and .08+.0151=.0951 : .08 : : 100 : 84.1 nearly.

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* Researches Chim. Phil. p. 287.

And, according to these indications, 100 parts of potash consists of about 84 basis and 16 oxygen.

In an experiment on the decomposition of water by the basis of soda, the mercury in the barometer standing at 30.4 inches, and in the thermometer at 52° Fahrenheit, the volume of hydrogen gas evolved by the action of .054 grains of basis equalled that of 326 grains of quicksilver.

Now, this, at the mean temperature and pressure, would require for its conversion into water .0172 of oxygen, and .054+.0172=.0712 : .054 : : 100 : 76 nearly; and, according to these indications, 100 parts of soda consists of nearly 76 basis and 24 oxygen.

In another experiment made with very great care, .052 of the basis of soda were used, the mercury in the barometer was at 29.9 inches, and that in the thermometer at 58° Fahrenheit. The volume of hydrogen evolved was equal to that of 302 grains of mercury, which would demand for its saturation by combustion at the mean temperature and pressure, .01540 grains of oxygen; and 100 parts of soda, according to this proportion, would consist nearly of 77 basis and 23 oxygen.

The experiments which have been just detailed are those in which the largest quantities of materials were employed. I have compared their results, however, with the results of several others, in which the decomposition of water was performed with great care, but in which the proportion of the basis was still more minute. The largest quantity of oxygen indicated by these experiments was, for potash 17, and for soda 28 parts in 100, and the smallest 13 and 19; and, comparing all the estimations, it will, probably, be a good approximation to the truth to consider potash as composed of about six parts basis and one of oxygen, and soda as consisting of seven basis and two oxygen.

VII. *Some general Observations on the Relations of the Bases of Potash and Soda to other Bodies.*

Should the bases of soda and potash be called metals? The greater number of philosophical persons to whom this question has been put have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination.

Their low specific gravity does not appear a sufficient reason for making them a new class; for amongst the metals themselves there are remarkable differences in this respect, platina being nearly four times as heavy as tellurium;* and in philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination, which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language.

Potasium and sodium are the names by which I have ventured to call the two new substances; and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an error; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country upon the method of derivation, and the one I have adopted has been the one most generally approved. It is, perhaps, more significant than elegant; but it was not possible to found names upon specific properties not common to both; and though a name for the basis of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalies.

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* Tellurium is not much more than six times as heavy as the basis of soda. There is great reason to believe metals of a similar chemical nature to the basis of potash and soda will be found of intermediate specific gravities between them and the lightest of the common metals. Of this subject I shall treat again in the text in the course of the following pages.

The more caution is necessary in avoiding any theoretical expression in the terms, because the new electro-chemical phenomena that are daily becoming disclosed seem distinctly to show that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision than a conviction of its permanency and truth.

The discovery of the agency of the gases destroyed the hypothesis of Stahl. The knowledge of the powers and effects of the ethereal substances may, at a future time, possibly act a similar part with regard to the more refined and ingenious hypothesis of Lavoisier, but, in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of chemistry.

Whatever future changes may take place in theory, there seems, however, every reason to believe, that the metallic bases of the alkalies and the common metals will stand in the same arrangement of substances, and as yet we have no good reasons for assuming the compound nature of this class of bodies.*

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* A phlogistic chemical theory might certainly be defended on the idea that the metals are compounds of certain unknown basis with the same matter as that existing in hydrogen; and the metallic oxyds, alkalies, and acids, compounds of the same basis with water; but in this theory more unknown princi-

The experiments in which it is said that alkalies, metallic oxyds, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner;* for distilled water, as I have endeavoured to show,† may contain both saline and metallic impregnations, and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds.

In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter.

The compounds of iron, of the alkalies, and the earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic,‡ and granitic rocks, there is a con-

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ples would be assumed than in the generally received theory; it would be less elegant, and less distinct. In my first experiments on the distillation of the basis of potash, finding hydrogen generally produced, I was led to compare the phlogistic hypothesis with the new facts, and I found it fully adequate to the explanation. More delicate researches, however, afterwards proved, that in the cases when inflammable gases appeared, water, or some body in which hydrogen is admitted to exist, was present.

* The explanation of Van Helmont of his fact of the production of earth in the growth of the willow was completely overturned by the researches of Woodward, Phil. Trans. vol. xxi. p. 193. The conclusions which Mr. Braconnot has very lately drawn from his ingenious experiments, Annales de Chimie Fevrier, 1807, p. 187, are rendered of little avail, in consequence of the circumstances stated in the text. In the only case of vegetation in which the free atmosphere was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances which might afford carbone, or various inflammable matters. Carbonaceous matter exists in several stones, which afford a whitish or greyish powder, and, when in a stone, the quantity of carbonate of lime is very small; in proportion to the other earthy ingredients it is scarcely acted on by acids.

† Bakerian Lecture, 1806, p. 8.

‡ In the year 1804, for a particular purpose of geological inquiry, I made an analysis of the porcelain clay of St. Stephen's, in Cornwall, which results from the decomposition of feldtspar of fine-grained granite: I could not detect in it the smallest quantity of alkali. In making some experiments on speci-

stant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutro-saline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements than to reduce them into simple elements.

VIII. *On the Nature of Ammonia and Alkaline Bodies in general, with Observations on some Prospects of Discovery offered by the preceding Facts.*

Ammonia is a substance, the chemical composition of which has always been considered of late years as most perfectly ascertained, and the apparent conversion of it into hydrogen and nitrogen in the experiments of Scheele and Priestley, and the more refined and accurate experiments of Berthollet, had left no doubt of its nature in the minds of the most enlightened chemists.

All new facts must be accompanied however by a train of analogies, and often by suspicions with regard to the accuracy of former conclusions. As the two fixed alkalies contain a small quantity of oxygen united to peculiar bases, may not the volatile alkali likewise contain it? was a query which soon occurred to me in the course of inquiry; and in perusing the accounts of various experiments made on the subject, some of which I have carefully repeated, I saw no reason to consider the circumstance as impossible. For, supposing hydrogen and

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mens of the undecompounded rock taken from beneath the surface, there were evident indications of the pressure of a fixed alkali, which seemed to be potash. So that it is very probable that the decomposition depends on the operation of water, and the carbonic acid of the atmosphere on the alkali, forming a constituent part of the crystalline matter of the feldtspar, which may *disintegrate* from being deprived of it.

nitrogen to exist in combination with oxygen in low proportion, this last principle might easily disappear in the analitical experiments of decomposition by heat and electricity, in water deposited upon the vessels employed, or dissolved in the gases produced.

Of the existence of oxygen in volatile alkali I soon satisfied myself. When charcoal carefully burned and freed from moisture was ignited by the voltaic battery of the power of 250 of six and four inches square, in a small quantity of very pure ammoniacal gas,* a great expansion of the aëriform matter took place, and a white substance formed, which collected on the sides of the glass tube employed in the process, and this matter, exposed to the action of diluted muriatic acid, effervesced so that it was probably carbonate of ammonia.

A process of another kind offered still more decisive results. In this the two mercurial gasometers, of the invention of Mr. Pepys, described in No. 14 of the Philosophical Transactions for 1807, were used with the same apparatus as that employed by Messrs. Allen and Pepys for the combustion of the diamond, and these gentlemen kindly assisted in the experiment.

Very pure ammoniacal gas was passed over iron wire ignited in a platina tube, and two curved glass tubes were so arranged as to be inserted into a freezing mixture, and through one of these tubes the gas entered into the platina tube, and through the other it passed from the platina tube into the air-holder arranged for its reception.

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* The apparatus in which this experiment was made, is described in page 214, Journal of the Royal Institution. The gas was confined by mercury which had been previously boiled to expel any moisture that might adhere to it. The ammonia had been exposed to the action of dry pure potash, and a portion of it equal in volume to 10980 grains of mercury, when acted on by distilled water, left a residuum equal to nine grains of mercury only. So that the gas, there is every reason to believe, contained no foreign aeriform matter, for even the minute residuum may be accounted for by supposing it derived from air dissolved in water.

The temperature of the atmosphere was 56°, but it was observed, that no sensible quantity of water was deposited in the cooled glass tube transmitting the unaltered ammonia; but, in that receiving it after its exposure to heat, moisture was very distinct, and the gas appeared in the air-holder densely clouded.

This circumstance seems distinctly to prove the formation of water in this operation for the decomposition of ammonia, unless, indeed, it be asserted, that the hydrogen and nitrogen gases evolved hold less water in solution or suspension than the ammonia decomposed; an idea strongly opposed by the conclusion of Mr. Dalton,* and the experiments of Messrs. Desormes and Clement.†

After the gas had been passed several times through the ignited tube from one gasometer to the other, the results were examined. The iron wire became converted superficially into oxyd, and it gained in weight $\frac{44}{100}$ parts of a grain; about four tenths of a grain of water were collected from the cooled glass tubes by means of filtering paper, and 33.8 cubic inches of gas were expanded into 55.3 cubic inches, and, by detonation with oxygen, it was found that the hydrogen gas in these was to the nitrogen as 3.2 to one volume.

It will be useless to enter into the more minute details of this experiment, as no perfectly accurate data for proportions can be gained from them; for the whole of the ammonia was not decomposed, and as the gas had been prepared by being sent from a heated mixture of sal ammoniac and quicklime into the air holder, it was possible that some solution of ammonia might have been deposited, which, by giving out new gas during the operation, would increase the absolute quantity of the material acted upon. In examining the results of M. Berthollet's‡ elaborate experiments on the decomposition of ammonia by electricity, I was surprised to find that the weight of hydrogen and nitrogen produced, rather exceeded than fell short of that of the ammonia, considered as decomposed, which was evidently contradictory to the idea of its containing oxygen. This circumstance, as well as the want of coincidence between the results and those of Priestley and Van Marum on the same subject, induced me to repeat the process of the electrization of ammonia; and I soon found that the quantities of the products in their relations to the apparent quantity of gas destroyed were influenced by many different causes.

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* Manchester Memoirs, vol. v. part ii. p. 535. 1786.

† Annals de Chimie, vol. xlii. p. 125.

‡ Memoirs de l'Academie, 1785, p. 324.

Ammonia procured over dry mercury, from a mixture of dry lime and muriate of ammonia, I found deposited moisture upon the sides of the vessel in which it was collected, and in passing the gas into the tube for electrization, it was not easy to avoid introducing some of this moisture, which must have been a saturated solution of ammonia at the same time.

In my first trial, made upon gas passed immediately from the vessel in which it had been collected into the apparatus, I found the expansion of one of ammonia vary, in different instances, from 2.8 to 2.2 measures, but the proportions of the nitrogen and hydrogen appeared uniform, as determined by detonation of the mixt gas with oxygen, and nearly as one to three in volume.

To exclude free moisture entirely, I carefully prepared ammonia in a mercurial air holder, and after it had been some hours at rest, passed a quantity of it into the tube for decomposition, which had been filled with dry mercury. In this case 50 parts became 103 parts by electrization, and there was still reason to suspect sources of error.

I had used iron wires not perfectly free from rust for taking the spark, and a black film from the mercury appeared on the sides of the tube. It was probable that some ammonia had been absorbed by the metallic oxyds both upon the iron and the mercury, which might again have been given out in the progress of the operation.

I now used recently distilled mercury, which did not leave the slightest film on the glass tube, and wires of platina. The ammonia had been exposed to dry caustic potash, and proved to be equally pure with that mentioned in page 87;—60 measures of it, each equal to a grain of water, were electrized till no further expansion could be produced; the gas filled a space equal to that occupied by 108 grains of water. The thermometer in this experiment was at 56°, and the barometer at 30.1 inches. The wire of platina transmitting the spark was slightly tarnished.* The 108 measures of gas, carefully analyzed, were found to consist of 80 measures in volume of hydrogen, and 28 measures of nitrogen.

The results of an experiment which I made in 1799† give the weight of 100 cubic inches of ammonia as 18.18 grains at the mean temperature and pressure. I had reasons, however, for suspecting this estimation might be somewhat too low; and, on mentioning the circumstance to Messrs. Allen and Pepys, they kindly undertook the examination of the subject, and Mr. Allen soon furnished me with the following data: "In the first experiment 21 cubic inches of ammonia weighed 4.05 grains; in a second experiment the same quantity weighed 4.06 grains, barometer 30.65, thermometer 54° Fahrenheit."

Now, if the corrections for temperature and pressure be made for these estimations, and a mean taken, 100 cubic inches of ammonia will weigh 18.67 grains, barometer being at 30, and thermometer 60° Fahrenheit; and if the quantity used in the experiment of decomposition be calculated upon as cubic inches, 60 will weigh 11.2 grains; but the hydrogen gas evolved equal to 80, will weigh 1.93‡ grains, and the nitrogen equal to 28.83.* And 11.2 grains—1.9+8.3=10.2, and 11.2—10.2=1, all the estimations being made according to the standard temperature and pressure. So that in this experiment on the decomposition of ammonia, the weight of the gases evolved is less by nearly one-eleventh than that of the ammonia employed; and this loss can only be ascribed to the existence of oxygen in the alkali; part of which probably combined with the platina wires employed for electrization, and part with hydrogen.

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* This, most probably, was owing to oxydation. When platina is made positive in the voltaic circuit in contact with a solution of ammonia, it is rapidly corroded. This is an analogous instance.

† Researches Chim. et Phil. p. 62.

‡ Lavoisier's Elements, p. 569. A cubical inch of hydrogen is considered as weighing .0239.

After these ideas the oxygen in ammonia cannot well be estimated at less than seven to eight parts to the hundred; and it probably exists in a larger proportion, as the gas evolved may contain more water than the gas decomposed, which of course would increase their volume and their absolute weight.†

In supposing ammonia a triple compound of nitrogen, hydrogen, and oxygen, it is no less easy to give a rational account of the phenomena of its production and decomposition, than in adopting the generally received hypothesis of its composition.

Oxygen, hydrogen, and nitrogen are always present in cases in which volatile alkali is formed; and it usually appears during the decomposition of bodies in which oxygen is loosely attached, as in that of the compounds of oxygen and nitrogen dissolved in water.

At common temperatures, under favourable circumstances, the three elements may be conceived capable of combining and of remaining in union; but at the heat of ignition the affinity of hydrogen for oxygen prevails over the complex attraction, water is formed, and hydrogen and nitrogen are evolved; and, according to these conclusions, ammonia will bear the same relation to the fixed alkalies, as the vegetable acids with compound bases do to the mineral ones with simple bases.

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* Researches Chim. et Phil. p. 9. From my experiments 100 cubical inches of nitrogen weighed, at the standard temperature and pressure, 29.6 grains.

† In the present state of our knowledge, perfectly correct data for proportions cannot probably be gained in any experiments on the decomposition of ammonia, as it seems impossible to obtain the absolute quantity of water in this gas; for electrization, according to Dr. Henry's ingenious researches, offers the only means known of ascertaining the quantity of water in gases.

Oxygen then may be considered as existing in and as forming an element in all the true alkalies; and the principle of acidity of the French nomenclature might now likewise be called the principle of alkalescence.

From analogy alone it is reasonable to expect that the alkaline earths are compounds of a similar nature to the fixed alkalies, peculiar highly combustible metallic bases united to oxygen. I have tried some experiments upon barytes and strontites, and they go far towards proving that this must be the case. When barytes and strontites, moistened with water, were acted upon by the power of the battery of 250 of 4 and 6, there was a vivid action and brilliant light at both points of communication, and an inflammation at the negative point.

In these cases the water might possibly have interfered. Other experiments, however, gave more distinct results.

Barytes and strontites, even when heated to intense whiteness in the electrical circuit by a flame supported by oxygen gas, are non-conductors: but, by means of combination with a very small quantity of boracic acid, they become conductors; and in this case inflammable matter, which burns with a deep red light in each instance, is produced from them at the negative surface.

The high temperature has prevented the success of attempts to collect this substance; but there is much reason to believe, that it is the basis of the alkaline earth employed.

Barytes and strontites have the strongest relations to the fixed alkalies of any of the earthly bodies;*—but there is a chain of resemblances through lime, magnesia, glucina, alumina and silex. And by the agencies of batteries sufficiently strong, and by the application of proper circumstances, there is no small reason to hope, that even these refractory bodies will yield their elements to the methods of analysis by electrical attraction and repulsion.

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* The similarity between the properties of earths and metallic oxyds was noticed in the early period of chemistry. The poisonous nature of barytes, and the great specific gravity of this substance, as well as of strontites, led Lavoisier to the conjecture, that they were of a metallic nature. That metals existed in the fixed alkalies seems, however, never to have been suspected.

In the electrical circuit we have a regular series of powers of decomposition, from an intensity of action, so feeble as scarcely to destroy the weakest affinity existing between the parts of a saline neutral compound, to one sufficiently energetic to separate elements in the strongest degree of union, in bodies undecompossible under other circumstances.

When the powers are feeble, acids and alkalies, and acids and metallic oxyds, merely separate from each other; when they are increased to a certain degree, the common metallic oxyds and the compound acids are decomposed; and, by means still more exalted, the alkalies yield their elements. And, as far as our knowledge of the composition of bodies extends, all substances attracted by positive electricity are oxygen, or such as contain oxygen in excess; and all that are attracted by negative electricity are pure combustibles, or such as consist chiefly of combustible matter.

The idea of muriatic acid, fluoric acid, and boracic acid containing oxygen, is highly strengthened by these facts. And the general principle confirms the conjecture just stated, concerning the nature of the earths.

In the electrization of boracic acid moistened with water, I find that a dark coloured combustible matter is evolved at the negative surface; but the researches upon the alkalies have prevented me from pursuing this fact, which seems, however, to indicate a decomposition.

From their analogy to ammonia, nitrogen and hydrogen have been supposed to be amongst their elements. It is singular, with regard to this class of bodies, that those most unlike metallic oxyds are the first that have been demonstrated to be such.

Muriatic acid, and fluoric acid, in their gaseous states, are non-conductors; and, as there is every reason to believe, that their bases have a stronger attraction for oxygen than water, there can be little hope of decomposing them in their aqueous solutions, even by the highest powers.

In the electrization of some of their combinations there is, however, a probability of success.

An immense variety of objects of research is presented in the powers and affinities of the new metals produced from the alkalies. In themselves they will undoubtedly prove powerful agents for analysis; and, having an affinity for oxygen stronger than any other known substances, they may possibly supersede the application of electricity to some of the undecompounded bodies.

The basis of potash, I find, oxydates in carbonic acid, and decomposes it, and produces charcoal when heated in contact with carbonate of lime. It likewise oxydates in muriatic acid; but I have had no opportunity of making the experiment with sufficient precision to ascertain the results.

In sciences, kindred to chemistry, the knowledge of the nature of the alkalies, and the analogies arising in consequence, will open many new views; they may lead to the solution of many problems in geology, and show that agents may have operated in the formation of rocks and earths, which have not hitherto been suspected to exist.

It would be easy to pursue the speculative part of this inquiry to a great extent; but I shall refrain from so occupying the time of the society, as the tenor of my object in this lecture has not been to state hypothesis, but to bring forward a new series of facts.

No original communication of greater importance to our country at this period can be published, than the following one on Merino Sheep, taken from the Transactions of the New-York Society for the Promotion of useful Arts. It contains the best account we have seen of this valuable breed of animals, and demonstrates the great advantage of their introduction into the United States. We are persuaded this paper will be read with uncommon interest by every American; and it would be wrong in us not to give it greater publicity than it has yet received. A few copies only of the volume of transactions from which it is taken have been printed, principally for the use of the members of the society, and none of them have ever been offered for sale. In our next number this respectable publication will be reviewed. In the mean time we think it our duty to recommend it to the booksellers to have it reprinted. It is a pity that so useful a work should not be extensively circulated.

EDITORS.

An ESSAY *on* SHEEP, WOOL, *and* WOOLLEN MANUFACTURES, *showing the Advantage of improving American Wool, by the Introduction of Spanish Sheep, &c. By* ROBERT R. LIVINGSTON, *LL. D. President of the Society for the Promotion of useful Arts in the State of New-York.*

I HAVE seen with pleasure that the agriculture of my native country has improved considerably, in the short space of four years, the time of my absence. Much, however, remains to be done before it acquires that perfection to which all nations should aspire, but which none that I have seen in Europe, Lombardy and South Holland excepted, have in any degree attained. England, though she boasts with reason some of the best farmers in the world, is but partially improved, many districts are ill cultivated, and old habits are not yet rooted out. I shall, as time and leisure admit, take occasion to lay before you such practices of the old world as I think may be advantageously adopted by the new; at present I wish to draw your attention to a subject which our probable rupture with some of the European powers, renders peculiarly interesting at this moment. I mean the improvement in our breed of sheep. It is upon this that we must rely for our independence of Europe, in an article of the first necessity.

The sheep of this State, taken collectively, are superior to almost any native race that I have seen in France, Holland, Lombardy or Italy, and very much resemble the south down sheep of England. In general, the sheep of those countries are about the size of ours; those upon the sea coast on the Channel and Lombardy are larger; those in Brittany much smaller. The wool of most of these breeds is inferior to ours in fineness, if we except the sheep upon the borders of Spain, some travelling flocks in the kingdom of Naples, and those in the small district of Berry. Anderson says, that wool sent from New-Jersey sold at the rate of the finest wool in England.

I believe in cold or temperate climates the inferiority of quality in the wool does not arise from climate, but from other accidental causes, which I shall point out. In France, I attribute it, first, to a neglect of those breeds that bear fine wool, and next, to the manner in which the sheep are kept.—They are folded in close pens, during the summer, all night, and part of the day. In winter they are crowded in small and ill aired houses; they are fed upon the commons, on the road sides, and on the stubbles, always confined close together by the shepherd's dogs, who are continually running round them, to prevent their straggling or touching the grain or meadows at the sides of them, for there are no fences. In dry seasons they are extremely pinched for food, and in the winter no green fodder or clover is prepared for them, and very seldom even hay; they are left to glean in the fields when there is no snow, and, when there is, to feed upon the coursest hay, or the leaves and branches of trees, which is frequently the only provender laid up for them. It is held by the British agriculturalists, that bad keeping makes bad wool. If the sheep are alternately well fed and starved, the wool will be of different strength and thickness, and, of course, unequal and of a bad staple.

That the climate effects no change in them, I infer from the great success that has attended the introduction of Spanish sheep, which, where they have been treated with a little attention, have so greatly improved in their size, form and fleece, without any change in the quality of the wool, that full bred rams, imported directly from Spain, may now be purchased in France at a much less price than rams from the national flock at Rambouillet; a race that were introduced about twenty years ago into France. Superfine cloth can only be made from Spanish wool, and that without mixture with other sorts, from which it differs so materially, that Anderson asserts, on the information of British manufacturers, that they cannot be wrought together. The different species of broad cloth are not made by mixture with British wool, but by Spanish wool of different qualities. The coarse cloths only are made of British wool. France, as well as all the rest of Europe, being dependent upon Spain for the wool used in their fabricks of fine cloth, made various attempts to introduce Spanish sheep. In 1766 a number were imported and distributed among the people of different districts. But as the general opinion was, that Spanish sheep could only thrive in Spain, that the wool would degenerate if they did not travel from the plains to the mountains, and from the mountains to the plains; and, above all, as the peasantry thought, that what they received without price could be of little worth, no attention was paid to keeping the race distinct, and, of course, little advantage resulted from the measure, except to a few enlightened farmers: But their experiments sufficiently proved the practicability of the project, and determined the government to make the attempt again, and to put the direction of the project into the hands of a distinguished agriculturalist.

Application was made to the king of Spain for permission to draw from his dominions a number of Merinos, the name by which the fine woolled sheep are distinguished. He not only permitted this, but ordered that they should be chosen out of the finest flocks in the kingdom. In the year 1786, they arrived at Rambouillet, the national farm. Instead of giving away the increase, as had before been done, they were annually sold, which, of course, put them into the hands of the richer and more intelligent farmers. At first they brought a very moderate price, but their superiority over the other sheep of the country, the great improvement in the wool that resulted from crossing the breed, were so manifest, and the evidence that experience afforded of their supporting the change of climate and treatment without any sensible change in the quality of their wool, rendered the demand for them so great, that they have considerably advanced in price. In 1796, the average price was 80” franks, about 16$; the last sale (April, 1805) the average price for a ewe was 250”, that is, about 50$; rams brought from 60 to 120$. This is the more remarkable, as by the last treaty between France and Spain, the former had a right to draw 5000 Merinos from the flocks of the latter, 500 to be chosen annually for ten years. Skilful shepherds were sent to select them, and France now possesses above thirty thousand of these sheep, by importation, and by natural increase, and yet the price of the stock at Rambouillet has been regularly on the rise. The late minister of the interior, Mr. Chaptal, has a very fine flock, consisting of 1200 sheep. As the sales at Rambouillet were over before I returned from Italy, I requested him to spare me five from his flock, to which he consented, provided I only took lambs, and not more than two rams, for which I was to pay 1500 franks, about 300$; this too was a very special favour. The shepherd I sent to choose them found the flock infected with the scab. I did not, therefore, think it prudent to take them, but left the money with a gentleman, who has promised to bring them out next spring, either from that flock, or those at Rambouillet. I should mention another circumstance, which proves that the wool does not grow worse in France when the stock from which they sprung was good. In April every year there is a sale of lambs of the preceding year, and of wool; the price of the latter was kept down by the artifices of the wool dealers, who pretended that it was inferior to Spanish wool. Some of the manufacturers, however, having for the two or three last years produced cloth at the exhibition, made of this wool, equal to that from the finest Spanish wool, the price has advanced to a par with the wool brought from Spain.

I should observe that the fine French cloths are finer and softer than those made in England, probably because very little of the finest Spanish wool goes to England, their import consisting of the second and third sorts, with some still coarser. The finest of the wool, to the amount of about three millions of pounds, is manufactured at the royal factories in Spain, and the remainder goes to France and Italy. The quantity of wool drawn from Spain by France was, before the revolution, about 4,000,000lb. but the manufactures having been ruined during the revolution, it was greatly diminished; what it is now I cannot declare. In the year 1786, England imported only three millions; but in the year 1796, the following is the state of the legal export from Spain, some is always smuggled into France and elsewhere.

France	600,000
England	6,000,000
Holland and N.	3,200,000
Italy	1,000,000
	10,800,000

Spain employs about three millions of pounds in her own manufactures. It may be proper here to observe, that all the sheep of Spain are by no means Merinos; but more of the stationary flocks are either what they call Chorinos, which are a large, hardy, coarse woolled breed, or a mixture between them and the Merinos. Part of this latter wool is also exported.

As my object in this essay is to endeavour to impress upon my country the importance of propagating this breed of sheep, it may be proper to show the value of this wool compared to that of other races, and particularly of that of England, and to remove some false ideas that have gone abroad relative to them.

The prices at Madrid for washed wool, in the year 1796, were as follows:

		"		"	c		*Cts.*
Leonise	from	5	to	5	4	about	100
Segoviense		5		4	60		88
Sorriens		4		4	25		86
Arragon		3					60

Anderson gives the value of wool in the London market about the same period, which, reduced to our money, stands thus: for the best

German	22 Cents.
Polish	25
British	17
White Persian	84
Red Carramanian	100
Spanish	98

It appears from this, that the finest Spanish wool is not carried to England, since the price there was below the price at Madrid about the same period. Gov. Pownal, in a letter to Arthur Young, 1788, gives the following as the average prices of British wool: coarse seven and an half pence, common eight and an half pence, fine eleven-pence the whole fleece; that is, reduced to our money, about, coarse 14 cents, common 16, and fine 20; the south-down, which appears to me to have the staple of our best wool, 1s. or 1s. and 9d. our money. This difference between the price of British and Spanish wool, is the more worthy our notice, because it enables us to combat the prejudices which so generally prevail among us in favour of British sheep. We should then stop and examine candidly how far these prejudices are well founded. Britain contains a great variety of sheep, from the long woolled sheep, bred in marshy grounds, to the small fine woolled sheep fed on the Welsh mountains. The Durham breed, which is, I believe, the largest, weighs about 260lb. live weight, and yields from 6 to 8lb. of wool; but this wool would be of little value, except where blankets or camblets are made; its value in England is about 14 cents the pound. A Spanish fleece, taking the flock round, will yield, exclusive of tags, about three pounds each, (mine give me near four) worth at least 90 cents, that is, for the whole fleece, 227 cents. The British fleece, at 7lb. which, stating it high, for a flock round, at 14, is 98 cents, less than one third of the value of the Spanish fleece, and yet the sheep weighs only about one third of the weight of the British sheep; and if, as I believe can hardly be doubted, animals of the same species eat in proportion to their bulk, the expense of the British sheep must be three-fold greater than that of the Spanish Merino; or, in other words, we may have three Spanish fleeces, worth together six dollars and 81 cents, at the same rate as one long woolled British fleece, worth only 98 cents: but this is not the only objection to these sheep; the strongest is, that we have few or no pastures adapted to them; they would then, of course, degenerate. Indeed, this breed is by no means the favourite breed in England. Those now generally sought for rich pastures are the Dishly, which weigh alive about 150 pounds when fat, and the south-down for lighter land. The Dishly is of the long woolled kind, and may give about 6lb. of wool, worth 16 cents the pound, so that the only advantage it has over the former breed lays in the carcass, which is, indeed, the principal object in England, on account of the low price of wool, and the high price of meat, circumstances which do not operate in this country, where wool is 50 per cent. higher than in England, and mutton 80 per cent. cheaper. I shall subjoin to this a note of all the breeds of England, together with the quantity and price of their fleeces, so that every farmer may compare their value, supposing even that they would not degenerate, with the value of Spanish sheep, which we know will retain their advantages in our pastures, and under our warm sky. Since, however, the rage in Britain for fine sheep (by which is meant sheep with fine carcasses, without regard to the wool) has so greatly increased, that 500 guineas are sometimes given for the hire of a ram for the season, no expense is spared in their food, and in proportion to their size must be the richness of their pastures, and winter-provender; turnips, tares, cole, potatoes, and rye, in the spring, are essentials, and sometimes even wheat must be sacrificed to them, if a backward spring should prevent the grass from coming forward in time for the lambs; for it is a rule always to keep them in an improving state, and this at so much expense, that Mr. Macro, one of the best farmers in England, asserts, that these fine breeds sink the value of the wool, and almost of the flesh in keeping, or, in other words, are not profitable to the farmer. How much less profitable would they be here, where so little attention is paid either to the summer or winter provision for sheep? And, indeed, experience has proved English sheep do not suit our climate or management. Numbers have been imported, but no trace of them remains after the third or fourth generation, unless it be near the sea coast, where the climate is moist, and where they can occasionally have recourse to the salt meadows. Add to this, that the flavour of all this factitious mutton (if I may use the term) is very much inferior to that of those which are capable of maintaining themselves in health and vigour with less care and attention.

It should be observed, that the prices above noted are near fifty per cent. less than wool of the same quality would bring in the United States, and that we discriminate less as to the quality. When, however, we manufacture more extensively, we shall necessarily distinguish the different qualities of wool, and, of course, Spanish wool will bear the same relation in price to fleeces of inferior quality that it does elsewhere. Should we, on the other hand, grow more wool than we choose to manufacture, (of which there are strong indications at present) since it is the only article that has ever risen in price this many years, Spanish wool would become an important article of exportation to every part of Europe, since we find that even in Spain, it is worth (the finest) about 8s. the pound. The markets of England would always be open to it, and, such is her demand, that if we could supply her consumption of 6,000,000lb. it would restore to our mutual advantage the balance of commerce between us.

The Merinos are rather smaller than the largest sheep we raise on the north of the Highlands. Those bred at Rambouillet are better made than those imported directly from Spain. The belly, cheeks, forehead and hind legs, are covered with wool that is short, curled and thick, and though extremely white when washed, yet brown at the extremities when on the sheep, particularly if folded or kept on any but the cleanest pasture. This is owing to the extreme thickness of the wool, which increases the perspiration of the animal, or rather perhaps to the greater quantity of grease that the wool contains, for in this circumstance it greatly differs from common wool, and it is never found harsh or dry. The wool on the thigh, which on our sheep is harsh and intermixed with hairs, in the Spanish sheep is soft and fine. From the thickness and evenness of the fleece the sheep is guarded against the wet and cold more effectually than our sheep, whose fleeces are looser, and whose bellies, after the second or third year, are only slightly covered with hairs instead of wool. Mr. Macro observes, that the most thrifty sheep throughout the winter, are those that have the thickest and most even coats. The fleece is entirely free from hair, which renders other wool harsh, and which never takes the dye perfectly. Having procured samples of all the wool that could be obtained in France, together with the improvement made in each by crossing the different breeds of sheep with Spanish rams, I shall lay them before the society, which will enable them to form their own judgment as to the quality. The inferiority in the size of the Merino to some other breeds, which some make as an objection, is in my opinion an important advantage, not only in sheep, but in every other stock not designed for the draft; because they will fatten in pastures in which larger cattle would suffer from the fatigue they must undergo in order to procure the food that is necessary for their support. This reasoning applies more strongly to sheep than to any other stock. They are generally kept upon high and dry pastures, that are frequently parched in summer, when fatigue is most irksome to them. To which we may add, that the fleece is not proportioned, as the food is, to the *bulk* of the animal, but to his *surface*, and a small sheep having more surface in proportion to his bulk, must also have wool in the same proportion; that is, a sheep whose live weight shall be 60lb. and who of course will require but one quarter of the food of a sheep that weighs 240lb. will, notwithstanding, have half as much wool (if the fleeces are equally thick) as his gigantic brother. The Merino has been found in France to be quite as hardy as the common sheep. At Rambouillet they have no winter feed but hay, and yet thrive very well; but what is more extraordinary is, that in Sweden, where the native sheep are extremely coarse woolled, the Merino has been naturalized without having in any sort changed the nature or quality of the wool, in the term of about thirty years since they were introduced. They have also been so well cultivated there, that though the consumption of fine wool has increased in Sweden, yet the importation of Spanish wool has greatly diminished.

My own experience has convinced me, as far as trials upon a small scale can do so, that the Merino is at least as hardy as our sheep. In the year 1802, I purchased from the national flock, at the vetenerary school, at Chalons, two young rams and two ewes. They cost me, delivered at Paris, (five miles distant) 1200". Charges, in getting them to a sea-port, maintenance till the ship was ready, and on board, (though the patriotism of the captain would not permit him to take any freight) brought them to almost double that price by the time they arrived at my farm. One of the rams I spared to my brother. The other, with two ewes, were treated exactly as my other sheep; they were fed on hay, and had no shelter. They brought me two lambs, and sheared eleven pounds of washed wool, near 3⅔ each. The next year the lambs came in January, were neglected, and died. The wool was not weighed. Last year one of the ewes was sick, and brought no lamb; the other dropped a ewe lamb: the five fleeces (from the three old sheep and two shearlings) when washed, weighed 18lb. besides the tags and waste wool. The two bred here are in every respect as fine sheep as the imported rams, without the smallest difference between the wool of one or the other. Upwards of 3½lb. wool will be thought a considerable yield from small sheep kept upon hay, in a flock with 90 other sheep. It equals the flock at Rambouillet, and exceeds by a pound the average of the Spanish travelling flocks. My present flock consists of two full bred rams, and three full bred ewes, including the lamb of this year; eight three quarter bred ewes, whose form and wool is already so like that of the imported sheep, that it is difficult to distinguish them. If any difference exists it is merely in the wool, on the extremity of the thigh, which is longer, and I think not so fine as that on the full bloods. I have also 18 half blood ewes and weathers, and about 20 country ewes that have run with the Spanish rams. All these sheep are kept together; they have a shed that they can go into, which is open on every side but the north and northwest; they have hitherto been fed with hay only, though I shall, just before they lamb, begin to give them bran and oats, having no green fodder. If any difference is discernable in the flock, it is in favour of the Merinos. One circumstance is remarkable; it is, that the half bred lambs have not only thicker, but longer fleeces than the common lambs, though these have naturally much longer wool than the Spanish Merino. This is an important circumstance, as it shows how necessary it is to cross the breed of sheep with those of a better stock. Should it be objected that the object here is to procure a great quantity of wool without regard to the quality, I would observe, that this might be true, if our landholders were an indigent peasantry, who sought only to be sheltered from the cold, instead of being what they are, and, I trust, always will be, men in such easy circumstances as to look beyond the mere necessaries, to the conveniences and comforts of life. Such men will take a pride and pleasure in being dressed in clothes whose softness and pliancy give warmth to the body, pleasure to the touch, and grace to the wearer; and they will be doubly proud of this, if it is the product of their own farms, and of the industry of their wives and daughters. That this is the fact, we may infer from the quantity of foreign clothes that are worn by farmers, whose own flocks might supply them abundantly with cloth of inferior quality, if they looked only to warmth in their dress; and from the attention that those among them, whom economy or patriotism induce to wear their own wool, pay to rendering the cloth as fine as the materials will admit. If, however, it should be thought, that cloth finer than the third quality of British cloth, which is generally worn by people in easy circumstances in our country, would be unnecessary, this may be procured by crossing our breed with the Merino, so as to have half, or even quarter bred Spanish sheep. This would add to the quantity as well as to the fineness of the fleece, and by sorting it we would have wool sufficiently fine for the master of the family and for his children, with a coarser sort for the domestics. In fact, the introduction of this breed of sheep will enable every man to have that quality of cloth that he likes best, without any additional expense, except that which arises from the spinning finer, which, being a domestic operation in most farm houses, will not be much regarded. In order to give some idea of the fineness and ductility of this wool, I cannot help mentioning, that Sir Joseph Banks, in a letter to Arthur Young, informs him, that the fleece of his Spanish sheep was spun into yarn 156,800 yards (about 90 miles) to the pound. Cotton, in machines, could only be spun into thread of 92,400 yards, and in jennies 144,000; which is much finer than can be spun by hand in England, though it is somewhat exceeded in France.

Though the climate of England, from its too great moisture, is not calculated for these sheep, yet it appears from this circumstance, that they do not soon degenerate even there; and I should suppose, from what I am going to mention, that they are turning their attention to the improvement of their sheep, by a mixture with the Merinos. About 20 rams, from the king of England's flock, (for he is himself a considerable farmer) were sold last year at auction; they fetched from thirty to forty guineas a piece. It would then be unpardonable in us, in our country, where they thrive so well, to neglect the propagation of them. I have hitherto viewed this question, with an eye to the domestic manufactures that we now carry on. But what prevents our manufacturing not only what is necessary for our families, but for the general consumption of our country? Why should not our city beaux, who look beyond convenience to elegance, find the gratification of their taste in the product of our own, rather than of foreign looms? The price of labour is the general objection. This is already diminished by carding-mills, which do the work better, and leave more women to employ in spinning, and of course must reduce the price of their wages. Mills have also been lately erected for spinning wool; and certainly if any wool can be spun fine in mills, it will be that which, from its softness and the evenness of its texture, most resembles cotton. Is it possible, that the price of labour can operate in an equal degree with the various charges that enhance the value of a piece of superfine British broadcloth, before it comes to the hand of the American consumer?

1st. The wool must be purchased in Spain, where a commission of not less than 5 per cent. must be paid. The mean duty upon the exportation of wool from Spain is 6d. sterling a pound, but upon fine wool much higher, because the duty is proportioned to the quality. This then is not less than 15 per cent. Transportation to the sea-ports, freight and insurance, port charges at landing, must amount to at least 5 per cent. more before it is stored in London. The merchant's profit cannot be less than 10 per cent. Thus, before the manufacturer receives the wool, it has paid 35 per cent. upon the first cost.—When manufactured, the clothier purchases it, sells it to the merchant—it must be packed to send off, and transported to London, or Liverpool, or Bristol; it pays a duty of 2 per cent. on exportation to America, though not elsewhere; freight, insurance, must be charged, and commission paid by our merchant to his London correspondent of 5 per cent. When it enters our port it pays 10 per cent. to the government. Twelve per cent. is the smallest profit our merchants charge. All these articles taken together, will add one hundred per cent. to the price of a pound of fine wool, manufactured into cloth, before it comes into the hands of one of our shopkeepers; and yet this is but a small proportion of the charge, for the commissions, duty, freight, &c. are not merely paid upon the original price of the wool, but upon every previous duty and charge, and upon every expense that attends the manufacturing of it. Thus a pound of fine wool, worked into superfine cloth, is worth, as it comes out of the loom, 6 $. The commission thus is upon the 6 $, and not upon one, the original price of the wool. If then we pay 100 per cent. on the price of the wool, before it reaches us, we pay in mere charges, independent of the price of labour, upwards of 600 per cent. beyond the price of labour upon the value of the raw material: But the price of labour here does not double that of England; of course, then, it is not the price of labour, but the want of fine wool that has hitherto prevented our manufacturing; and this every farmer knows, for all manufacture their wool rather than sell it, though they might buy coarse cloths relatively cheaper from the British merchant than fine ones, because the native wool of England, which serves to make such cloths, comes 35 per cent. cheaper to the manufacturer than the Spanish wool; and wool of the description which answers for such cloths, is 50 per cent. cheaper in England than in America: yet, even such cloth, we find a profit in manufacturing for ourselves. How much greater then would the profit be, upon the working of fine wool, where the labour would be but little advanced, and the value more than double? Let any man make this simple experiment; let him sort his wool with attention, and pick out that only which will make the finest cloth to be made from our wool; let it be carded, spun and wove by the same persons, and at the same rate that his coarse wool is fabricated; he will find, that one will give him cloth worth about 10s. our money, or 20s. if it was the breadth of English cloth; while the other will give him cloth only of 6s. and 6d. at the same expense, and demand more wool. If then cloth at 6s. and 6d. per yard pays him 2s. and 6d. for his wool, cloth of 10s. must pay him 7s. and 6d.; but if, instead of the wool of our sheep, he has Spanish sheep, not the sorted only, but the whole fleece will be so much finer than even his sorted wool from a common flock, that with exactly the same labour that he has expended upon his cloth of 6s. and 6d. the yard (yard wide cloth,) he will have cloth worth at least 15s. that is, he will sell the whole of his wool (supposing, which is the case, a pound of wool would make a yard of cloth, and the expense of spinning, weaving, &c. to be 5s.) at 11s. per pound. I suppose in this case the wool to be all spun equally coarse, yet the difference in the softness and suppleness, and in the wear, for the softest cloths not only wear longest, but are much warmer than those which are harsh, would make the difference I have stated in the price. I again then repeat, that it is not the high wages, but the want of good wool that has stopped the progress of our manufactory of cloth. It is true, that we have not as yet the quantity of wool that is necessary to supply our wants; but how long should we find this deficiency, if, without any diminution in the quantity, or without any additional expense, the price of our wool was doubled? I confess, that since the introduction of Spanish sheep, I have become so sanguine as to look forward to a period, and that too at no great distance, when this, and the Eastern States will not only manufacture cloths in sufficient quantity for their own consumption, but for that of their neighbours. It is certain that no country in Europe is better calculated for fine woolled sheep than this and the neighbouring States. The price of land is comparatively low; our grounds are high, and well watered, and our pastures good; our common grass is of the best and richest kind; the poa pratensis, and poa trivialis (which we call spear grass, and blue grass) are the most common, and most nutritious of all grasses. White and red clover abound, since the introduction of gypsum, in fields that were burned formerly during the summer; these may be made into hay for winter provender, at half the price that it would cost in the moist climate of Britain. Add to this, that our fields are enclosed, which saves the expense incurred by the constant attendance of shepherds. Our sheep have few maladies; many that are common in Europe are unknown here. There the rot sweeps off whole flocks. I never saw or heard of the rot in this State. Indeed, I know of but one disorder that is common among our sheep, the scab. This seldom is seen where the sheep are well kept, and is easily checked by removing the infected sheep as soon as it is visible. I have found it useful, when the snow lays long on the ground, to carry pine or cedar boughs into the fold, which they eat readily. When these cannot be conveniently procured, tar spread thinly over a board, and strewed with salt, is a good substitute.

The Spanish shepherds, when they perceive, by the wool's rising, the commencement of the scab, they pull out the lock, and, taking salt in their mouths, drop the spittle on the bare spot, which, they say, will stop its progress. The best method, however, when it is only partial, is to wash the part with soap, and to anoint it with an ointment of grease and turpentine. Mercurial ointment, rubbed on the sore, or on the hams of the sheep, is a certain remedy. The sheep should, in this case, be kept dry for some days. This has also the advantage of freeing them from lice or ticks. When the sheep are sheared, a good washing with soap suds, and a weak decoction of tobacco, will free them from the seeds of the scab. It is thought essential not only to the health of the sheep in Spain, but to the beauty of the wool, that they should have a full supply of salt. While I am speaking of the maladies of sheep, let me mention one that occasions more destruction among them than all the others put together—the bite of dogs. This animal is an absolute nuisance in the old settled countries, however useful he may be in the new ones. Nothing can be more vexatious, than, after a man has laboured for years to have an improved flock, to see them destroyed in one night: yet this has happened to me more than once, sometimes from my own dogs, and sometimes from those of others. The remedy for this evil lays with the legislature; a heavy tax upon all dogs that were seen without a collar, containing the master's name on a brass plate, and permission to kill them; together with a light tax upon all dogs furnished with collars, would raise a considerable revenue, and diminish their number. The master should in every case be answerable for the damage done by his dogs. Without this few people will be at the expense of 100 $ for a ram, besides the risk and trouble of importing him. You, gentlemen, will judge how far a matter so material to our agriculture and manufactures will merit the attention of the legislature.

In England, where more attention is paid to the beauty of the form and the early fattening, than to the wool, and where in these points they have attained an astonishing degree of perfection, it is thought essential to provide such food as will furnish plenty of milk to the lambs, and so to manage, that they never fall off, but are kept in a growing state till they are fit for the knife.

Their winter food is turnips fed on the ground, potatoes, cabbage, &c. winter vetches, tares, rye sown early, are the spring provision. I see no reason why all these may not be obtained here on easier terms than in England. The land and taxes are lower, though labour is somewhat higher. Turnips may, I think, be raised without any other expense than the seed. When the last ploughing is given to the Indian corn, harrow it smooth and sow turnips. The largest may be drawn, and the remainder fed on the ground when it is bare in the winter or spring. Pumpkins would, I believe, keep the flock in fine heart till January, and, if preserved from the frost, to a much later period. I have seen them sold in Paris through the whole winter. Vetches sown on light land, and dressed with gypsum, would yield a certain crop, and give the ewes a flush of milk, and thus save the pastures, that are injured by early feeding, and preserve the grass for the dry season. Wheat or rye sown early, and turnip seed with it, has been tried to advantage. The turnips are fed off when the ground is dry or frozen, and the grain, though fed down, has been benefited by the trampling and dung of the sheep, more than to compensate this inconvenience.

I should, before I conclude, say something upon the migration of the Spanish flocks, to which the fineness of their wool has been attributed. The travelling Merinos of Spain amount to above 5,000,000; they are driven (to the great injury of agriculture) from very remote parts of the kingdom, to the mountains, where they pass the summer, and are wintered in the plains. This keeps the wool from degenerating, by saving the sheep from the fold, and affording them that shade, water, and nourishment that they could not meet with in the dry pastures of Spain during the hot season. But experience has demonstrated, that this is not essential where they are well kept on the plains. There are many stationary flocks of Merinos, in Spain, whose wool is not inferior to that of those that travel. On the other hand, there are large flocks of sheep in the north-east of France, that do travel to the mountains during the summer, and yet bear very coarse fleeces. The Merinos introduced into France and Sweden never travel, and yet their wool has not degenerated.

I fear, gentlemen, that I have trespassed too long upon your patience, but I confess to you that I consider the improvement of our flocks by the intermixture of this breed, as an important object in agriculture, and as one that must form the basis of our woollen manufacture. Happily we have the means of extending this improvement. Col. Humphries has imported from Spain or Portugal a large flock of Merinos, and, I am told, lets out the rams to such as may require them. My small flock from France, together with those I expect out in the spring, will extend the breed to the northern parts of the State. Some other gentlemen have rams, and the prohibition upon the export from France having been taken off last autumn, more will probably be imported. Even the half blooded rams will contribute greatly to improve our flocks, and of these I presume many may now be procured. The samples of wool which I procured while I was at Paris for your inspection, will show the progressive improvement by crossing with all the different breeds of sheep that could be got in France.

Having been among the first to introduce the use of gypsum and clover in this State, and, by precept and example, to enforce a system which I have now the satisfaction to find general, in spite of the prejudices by which it was at first opposed, I flatter myself with the hope of being also instrumental in effecting another important improvement in agriculture, and of laying the foundation for a useful manufacture, in fewer years than it has taken to introduce the gypsum; which, indeed, by the improvement it has made in our grass grounds, has laid a foundation for the amelioration of our sheep. It is to the substanthe farmers that we must look for the first steps toward this improvement. They will not be deterred by the advances which the first cost in the selection of the best ewes and rams may occasion. They will not suffer them to degenerate through want of proper pastures in summer, and good clover hay, with potatoes and turnips, in the winter. And by wearing the wool of their own flocks, they will convince their fellow-citizens of the practicability of supplying their wants with cloths, equal, at least, to those of foreign manufacture, and at a much inferior price. It may, indeed, till we have made some further advances in dying and dressing, want that gloss which it receives in foreign fabrics; a gloss which wears off in a few days, and is known, without adding to its softness, warmth or beauty, to injure its strength.

REVIEW.

The Planter's and Mariner's Medical Companion; treating, according to the most successful Practice, 1. The Diseases common to warm Climates and on Shipboard. 2. Common Cases in Surgery, as Fractures, Dislocations, &c. 3. Complaints peculiar to Women and Children, &c. By JAMES EWELL, *Physician in Savannah.* 8vo. pp. 322. *Philadelphia.* Bioren. 1807.

> Melius est aliquid nescire quam cum periculo diſcere.
>
> ST. JEROME.

FIELDING endeavours, in one of his essays, to prove that an author will write the better for having some knowledge of his subject. This is perfectly lawyer-like, to take the wrong side of a question through choice, to show how ingeniously he can argue; or perhaps indeed in law the proposition may be true, as it is affirmed by some to be so in philosophy. In medicine the case is precisely reversed, those writers being by far the clearest who know least about the matter. For those authors whose minds are deranged by study and reflection, write so obscurely, and describe diseases so vaguely, that they candidly acknowledge that penetration, judgment, and God knows what besides, are necessary in those who attempt to apply their doctrines and principles to practice; whereas, popular writers, as they are termed, whose minds have never been perverted by reading, lay down such plain and ſimple rules, that no man can mistake a complaint, or err in the application of the proper remedy, provided he can understand bad English. While the former, who, for the sake of distinction, are called professional writers, as their works are intended for the profession excluſively, condemn their votaries to expend their money and waste their time in the purchase and perusal of numerous books, which are likewise very costly; the latter condense the whole into one small volume, which their readers can very conveniently carry in their pockets, if their heads are not sufficiently capacious. So much have these popular writers simplified the practice of physic, that nothing more is necessary than to be furnished with one of these books, which may be very conveniently carried about, and when a patient is afflicted with any complaint, dysentery for instance, all that is requisite is to look in the index for the word dysentery, then turn to the page pointed out, and give the medicines there directed. Can any thing be easier than such practice?

The success of these writers with the public has been commensurate with their merit, the sale of some of their works having been very extensive. This general diffusion of medical knowledge has been and is likely to continue of the greatest advantage to the poor, as in consequence of it, they can obtain medicine and advice gratuitously from their richer and better instructed neighbours. The rich indeed do not derive the same essential benefits from it, for, though very willing to prescribe for others, they are not so fond of administering to themselves. Occasionally it is advantageous even to them, as happened in the following instance: A gentleman to the southward was appointed overseer of the poor for the parish in which he lived, and, as he possessed a copy of Buchan's Domestic Medicine, he supposed that he could very easily save the expense of paying a Doctor by attending the paupers himself. Accordingly, he sallied forth with Buchan in his hand, to put his design into execution. At the next annual meeting of the overseers to take into consideration the condition of the poor, none could be found! the only one left having runaway to save his life. Thus the rich were relieved from the charge not only of doctor's fees, but also of the poor-rates.

Nothing, however, can be so good but that some discontented minds will be dissatisfied and cavil at it. Thus it is with country physicians in the present case. They say, that where a servant or some inferior member of a family is sick, that he is dosed until the master or mistress becomes alarmed, when a messenger is dispatched after the doctor, "to come as fast as he can, for that such an one is dying;" which prognostic the doctor frequently finds fulfilled on his arrival, or nothing is left for him to do but to confirm the sentence; whereas, had he been sent for a few days sooner, the life of the person might in all probability have been saved.

These practitioners likewise assert, that the very general prevalence of this species of quackery, if not originally caused, is much increased by popular works on medicine. And, moreover, that even where no mistake is committed on the part of the person prescribing, as to the nature of the disease, that much mischief may be done in consequence of the improper directions given by these authors.

This last complaint at least is, we are sure, totally inapplicable to Dr. Ewell's work, only see his manner of treating injuries of the head for example: "As it will be fair," says he, page 218, "to conclude from the deprivation of the senses,* that the brain may have sustained some injury—if the patient be of a plethoric habit, a moderate bleeding will be required, after which the bowels should be evacuated by purgative medicines, or glysters. One or two stools being procured, and, if possible, the warm bath used, the anodyne sudorific drops† should next be exhibited, to produce perspiration and *excite absorption of the extravasated blood;* and this mixture to be continued in doses of ten or twelve drops every four or six hours *until the patient is out of danger*, taking care to keep the bowels open." Should the scalp be lacerated, fomentations and poultices are directed so as "to induce suppuration, which, being established, a *relief of all the symptoms* will occur." Can any directions be more plain, more simple, or more erroneous? But comments would insult our readers. They no doubt are satisfied with the specimen we have given, and will gladly dispense with any farther extracts from the "Medical Companion." As to the style of the performance, it is faulty and incorrect in the extreme; but this blemish is lost in the magnitude of more important errors.

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* He is speaking of those cases in which a patient is rendered insensible from some accident.

† Composed of one-third laudanum and two-thirds antimonial wine.

It is not to be inferred from what we have said, that we condemn the exhibition of all medicines by persons who have not received medical educations. We by no means think that a physician is to be applied to on every occasion, to prescribe some simple remedy for some slight complaint; but we reprobate the idea of placing the most active and poisonous drugs of the shop in the hands of old women, and men perhaps more ignorant. We hold it absurd to suppose that such persons can acquire correct medical information from the best books the profession affords. As for the Medical Companion, and similar works, whom can they teach?

Principles of Surgery, Volume 3d, Part 1st.—Containing a Series of Cases calculated to illustrate chiefly the Doctrine of Tumors, and other irregular Parts of Surgery, and to instruct the young Surgeon how to form his Prognostics and to plan his Operations. By John Bell, *Surgeon. London: Printed for* Longman, Hurst, Rees *and* Orme; T. Cadell *and* W. Davies. 4to. pp. 298. 1808.

MR. BELL has been so long known to the public as an author, that we shall not detain our readers with a dissertation on his merits or defects. To us he appears to be a surgeon of extensive reading, and an anatomist of profound skill, to be bold and dextrous as an operator, and, if not always correct, uniformly pleasing and animated as a writer. In the description of diseases and operations he is unrivalled by any modern surgical author. He is as candid in confessing his own errors, as he is acrimonious in pointing out those of others. In argument he is diffuse, and frequently incorrect; hence he brings forward opinions which he cannot maintain, and prefers charges which he cannot support. He is sometimes inconsistent and contradictory, from the inattention and rapidity with which he writes. He follows no system, but writes his "Discourses" upon what subject he thinks proper, and digresses when he pleases. Notwithstanding the faultiness of his language, he possesses, in an eminent degree, the art of engaging the attention of his readers; and we are acquainted with the works of no medical author whose productions are equally voluminous and equally interesting. The volume before us, in particular, is replete with valuable matter, and interesting cases and dissections, which we will now lay before our readers, premising that it is impossible, in some instances, to do justice to the descriptions of Mr. Bell, on account of the numerous and admirable plates with which they are accompanied.

The following is the chapter of contents; its conciseness, and the total want of an index, are great imperfections, reference to any particular passage or fact in the book being very tedious and difficult.

"Discourse 1. On the excellency and importance of this department of surgery.
2. On the unlimited growth of tumor; the facts proved by various examples, and inferences of very general application in practice deduced from it.
3. Of tumors of the bones.
4. On tumors of the nostrils, gums, and throat.
5. Of the various operations practicable in the several stages of polypus. Of the first stage, or, of small and incipient polypi.

Second stage of polypus.
Of the third and last stage of polypus.
6. On tumors of the gums, lips, cheeks, and throat.
7. Of Salivary tumors.

"Memoir on the surgical diseases of the poor."

Our readers, we presume, are sufficiently aware of the "excellency and importance of this department of surgery," or if they are not, they will be after perusing some of the cases detailed below. With regard to the formation of tumors, we cannot agree with the author, who thinks they are caused by a mere increase of that regular and healthy action by which the body is nourished. Tumor then would be a simple augmentation, without a change of structure. How then are scirrhi formed? Is not the texture of a gland totally changed when it is affected with scirrhus? Some tumors appear to be nothing more than an increased deposition of matter, without a corresponding increase of absorption; but this is surely not the case in all tumors. With regard to the unlimited growth of tumors, the author relates two cases in proof of his position, in which, indeed, the tumors had acquired very great size; but does it follow that those which were of a totally different nature would likewise become as large if undisturbed? Mr. Bell likewise contends, that parts which are distended thicken in proportion to the distention. This is partially true, but false as an universal principle of the animal economy. If the distention causes a slight inflammation in the part, then this thickening takes place, otherwise not, except in the case of the impregnated uterus, which, not being diseased though enlarged, obeys not the laws of diseased action. The diseased coats of an artery, in a case of aneurism, thicken at first, but ultimately they become thinner, and rupture, the deposition not increasing in proportion to the distention. Hence it would appear that Mr. Bell's reasoning is too general, his principles too broad, and are true only to a certain extent. The rule of practice, however, which he deduces from his principles, is, as he says, incontrovertible, viz. "That no suspicious tumor, seated in a dangerous part, should be permitted to grow, as mere increase of size will produce the same effects as the greatest malignity."

In his "discourse on tumors of the bones," he describes that disease called osteo-sarcoma, in which the internal structure of the bone is entirely destroyed, and a fatty secretion supplies its place. The following case contains a description of the disease, and all the difficulties attending an operation for its cure.

But before relating the case, we must premise that Mr. Bell "professes not to know the stage and period of growth at which such a tumor may be extirpated, if ever such an operation be practicable." Amputation even, except where the disease is caused by some accident, he considers as doubtful; but he admits we have no other resource.

"A labouring man, about forty years of age, sallow, lean, and meagre, presented himself with a tumor of an enormous size, and of an anomalous character, partly solid, partly cartilaginous, occupying two thirds of the fore arm, from the wrist upwards: the hand was sound, and all its joints limber, the wrist bended, and the fingers moved easily; it was from pain only, and weakness, and the incumbrance of so great a tumor, that he could no longer work: the tumor seemed also to move freely, whence it seemed possible to dissect it away, and save the joint; and the surgeon, a man whom I respect as a man of learning, skill, and consummate prudence, was induced to begin a partial operation, a dissection of the tumor, from a sincere desire to preserve the right hand of a poor labourer.

"But here you are to take notice, (and I should put no value on a case which did not convey some practical lesson) how unexpectedly we are sometimes involved in great perplexities from reflecting too slightly on the nature of a tumor: a tumor of this singular complexion, any tumor, indeed, which requires an operation, should be so particularly examined, as to enable the surgeon to prognosticate every thing that could occur, and to describe the disease before amputation, as precisely as if it lay already dissected before him: much of what will be found on dissection may, in almost every case, be anticipated, and every such anticipation will be happy for the patient, and creditable for the surgeon. The surgeon should, at least, ascertain the general character of the tumor; yet, I question whether, in this case, it was absolutely known, that the tumor was at all attached to the bone; that it was merely a tumor of the radius, assuredly not.

"Little is to be learnt, even after much inquiry, from those of the lower orders concerning the early stages of their diseases. This, perhaps, was of a nature originally malignant, but certainly irritated by neglect at first, and, in the end, by imprudent advice, and rash applications: the man had, about six or eight months before applying for assistance, first observed the disease, in the form of a circumscribed swelling, rising upon his wrist, gradually increasing, and becoming daily more painful: he imagined it right to apply poultices, and, after some time, brought it happily, as he imagined, to a suppuration; but, as it did not heal, a mischievous old woman undertook the cure, cramming it with tents, and acrid and corrosive powders, and making so very free with the lancet, that he narrowly escaped dying of a hemorrhagy, caused probably by the erosion, or wounding of one of the veins above the wrist. The tumor was, at the time of the operation, enormously large; it was, at the lower and bigger part, of a dusky brown; but its upper and smaller end, of a fresher colour, with a wide and open ulcer, bleeding at times, and disposed to throw out a luxuriant fungus; to suppress the growth of which was, perhaps, part of the old woman's intention in applying the escharotics, if intention of any kind can be imputed to so ignorant a creature. The veins, as is usual in boney tumors, were far from being conspicuous even in this part.

"This poor man having willingly assented to any operation, however lingering or painful, which might save his hand, the dissection was carried all around the tumor, and into its central parts; before the surgeons present were undeceived. As the radius turns vertically like a spoke or spindle, it turns without any apparent motion, except in the parts connected with its lower end; the hand turns freely along with the radius, so that we never suspect, until we become acquainted with anatomy, that it is by the spoke-like motions of the radius that the hand moves; it seems moveable in itself by its own immediate joints. This tumor, in like manner, moved easily, could be turned upwards and downwards, so that the surgeon never once suspected that the motion was in the radius, or that the tumor was fixed and made a part of that bone; it seemed moveable, and, doubting, he began to extirpate it, by drawing a long incision round its root on the side of the ulna: but, finding it difficult with this limited incision to dissect the tumor, he prolonged the incision, continuing it over the back of the hands to the knuckles, in the direction of the extensor tendons. He then dissected more freely, and continued separating the skin from the tumor, till he came to a thick and solid sack, which seemed to consist of the muscular fibres and aponeurosis of the supinator quadratus muscle.*

"He continued this dissection, separating the thick and solid sac from the interosseous ligament, till he could go no farther: finding that it terminated then in a solid and osseous basis, he now plunged intrepidly into the heart of the tumor. In cutting into the heart of the tumor he found that he had opened a very large sac, not firm only, but osseous: but still as he was penetrating into it at one side, viz. at the side nearer the ulna, with which the tumor was manifestly unconnected, and at the greatest possible distance from the radius, from which the tumor, in fact, arose; he continued still unsuspicious, and persevered in dissecting away what he imagined to be a common tendinous sac, ossified only at certain points: he made thus a large opening into the tumor, felt its cavity full of loose and fatty bodies, pushed his fingers under the extensor tendons into the deepest part of the sac, began to hook out the fatty tubercles with his fingers, and, at last, bailing it out with his hand (for the cavity was large enough to admit his hand), hooking with his fingers, and catching the fatty masses in his palm, he so far emptied the cavity as to be able to search with his fingers in every direction, and then found, to his utter confusion, the ball of the carpus formed by the scaphoid and lunated bones at the bottom of the cavity, bare. He was now, for the first time, undeceived, and knew what sort of disease he had to contend with: he was now conscious that the radius was diseased, the joint destroyed, the original bone ulcerated and destroyed. There was no proceeding with the operation, and no stopping here; amputation was therefore proposed, assented to by the patient, and performed."

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* There is no such muscle mentioned by any author, not even Mr. B. himself; nor, after a careful dissection, have we been able to find it. *Ed.*

In page 66 a case is related from Severinus, in which this disease attacked the phalanges of the four fingers, which was cured by amputating the fingers only, where they articulate with the metacarpus; the thumb likewise was preserved.

Several other cases are related of bones affected with this disease; but it would exceed our limits to insert them.

The fourth discourse treats of "tumors of the nostrils, gums, and throat." He begins with polypi of the nostrils. Before explaining the opinions of our author upon this disease, we must first exculpate Mr. Pott from a charge brought against him by Mr. B. which we think unfounded. He says, that the fair and logical conclusion from what Mr. Pott advances, is, "that polypus is a disease which, if mild, should not be tampered with; if malignant, cannot be cured." Mr. Pott says, indeed, that malignant polypi cannot be cured; but the benign sort which he describes are fit for extraction, which ought to be done, as he expressly states. Few surgeons, we believe, have written less from imagination than Mr. Pott, and to few authors is the profession more indebted.

"Polypus," says Mr. B. "is a disease never mild, and never malignant; time, and the natural growth of the tumor, and the pressure which it occasions within the soft and boney cells of the nostrils and jaws, must bring every polypus to one invariable form in its last and fatal stage. It is, indeed, a dreadful disease; but, like every other, it becomes so by a slow progression, and advances by gradations easily characterised. It is at first a small, tight, and moveable tumor, attended with sneezing, watering of the eyes, swelling in moist weather, descending with the breath, but easily repressed with the point of the finger, void of pain, and in no shape alarming; and it is easily extracted too, so as for a time to clear the passage for breathing! Yet this little tumor, simple as it may appear, is the germ of a very fatal and loathsome disease; and this easy extraction, the very cause often of its appearing in its most malignant form: the more easily it is extracted, the more easily does it return, and when it does return, it has not, in truth, changed its nature; it has not ceased to be in itself mild; it is then to be feared, not from its malignity, but from its pressure among the delicate cells and membranes of the nose; it soon fills the nostrils, and obstructs the breathing, and causes indescribable anxieties: the patient is all night with his mouth open; during sleep he is harassed with fearful dreams, and when he wakes his mouth and throat are parched and dry. The tears are obstructed, and the eyes become watery from the pressure on the lachrymal sac; the hearing is in like manner injured from pressure against the mouth of the Eustachian tube; the voice is changed, and its resonance and tone entirely lost, by the sound no longer passing through the cells of the nose and face; the swallowing is in some degree affected by the tumor depressing the soft palate, the pains arising from such slow and irresistable pressure are unceasing; from the same pressure the bones become carious, and the cells of the face and nose are burst up by its slow growth.

"The bones and membranes now plainly ulcerate, a foul and fetid matter, blackened with blood, distils from the nostrils, and excoriates them, and, by passing partly down the throat, occasions diarrhœa: the blood vessels next give way, and sudden and impetuous hemorrhagy weakens the patient; the teeth fall from their sockets, through which a foul and fetid matter issues from the antrum.

"Now the disease verges towards its conclusion. The patient, conscious that the tumor lately so mild and moveable, has degenerated into a mortal disease, is resigned to his fate, and no flatteries of his friends, nor soothing words from his physicians can longer deceive him. In the night he starts from his sleep in horrible dreams, and with a sense of suffocation, and frequent hemorrhages bursting out from time to time, reduce him to such extremity of weakness, that for several days he is not able to crawl from his bed, and when he rises from it, he hangs over the fire cold from loss of blood, pale as a spectre, his lips colourless, and his face like wax, yellow and transparent: he hangs his head forward, resting it on his hand, and moving it incessantly from side to side from the intolerable pain, the saliva distilling from his mouth, and the foul matter dropping from his nose. In this state he survives a few weeks desolate and hopeless. During the last days of his illness, he lies in a state of perpetual stupor, and dies lethargic."

Such is the description given of the progress of polypi by Mr. Bell; and from the force and energy with which he writes, it is evident that he describes from nature. In speaking of the cause of polypi, he contradicts, in this book, what he said in his anatomy with respect to their sometimes being caused by picking the nose. In speaking of the spongy bones, he says, "their point forms that projection which is touched in picking the nose, and from that indecent practice *very often* serious consequences arise; *for in many instances polypi* of the lower spongy bones, which can be fairly traced to hurts of this kind, grow so as to extend down the throat, causing suffocation and death.” Whereas, in this book, pages 102 and 103, he abuses and ridicules those “who can imagine this disease, polypus, to have any relation to picking the nostrils,” and is at a loss to imagine how any person, writing even at a venture about surgery, could suppose any such thing! The contradiction to be sure is not material, but it shows the inattention with which Mr. B. occasionally writes. It is unnecessary to introduce his arguments to prove that polypi are never malignant; the causes which he assigns are certainly sufficient to produce all the symptoms which polypi occasion; why then suppose them malignant? He likewise contends that there is generally more than one polypus, sometimes in both nostrils, and introduces a case from Mannè, in which almost the whole Schniderian membrane was covered with them.

With regard to the manner of noosing nasal polypi with Levret's tubes, or in any other manner, our author considers it as next to impossible to succeed. “I have,” says he, “seen such an operation practised fifty times by men of various degrees of ingenuity and skill; some awkward, some perfectly dextrous; but never have I seen this method succeed.”

But where the polypus projects backward, so as to depress the soft palate, and may be distinctly seen and felt at the back part of the fauces, then may it be noosed with absolute certainty, in a manner hereafter to be described. But in the former case, though they cannot be noosed, they may be torn out with forceps, and their recurrence prevented, by the application of caustic to their roots: so that in one of these two ways every polypus may be cured, except it has arrived at its last and fatal stage. In extracting polypi with forceps, our author is particularly careful to ascertain that the nostril is entirely free; or should it happen, that there are some excrescences so low down in the nostrils that they cannot be reached with the forceps, he then employs a knife, which is long, slender, and somewhat crooked;

but its cutting edge short, to prevent injuring the narrow passages into which it is introduced. As to hemorrhagy, he says, there is little danger of its proving troublesome. Should this happen, it may be suppressed in the same manner that other hemorrhages from the nose are, that is, by drawing up a plug behind the palate, so as to stop the posterior openings of the nostrils, at the same time that the anterior are closed. After having done as much with the knife and forceps as they can do, Mr. B. proceeds to apply caustic to the roots of the polypi which have been removed, which he does with great boldness, little solicitous about destroying a portion of the membrane of the nose, or even some of the bone. His manner of applying it is as follows: " First I fold," says he, " a piece of lint twice or thrice, and give it a triangular form, and after pounding the caustic, I mix it with water into a paste, and spread it on the lint,* and then bend it over the point of a probe, or directory rather, the big obtuse point of which carries it and deposits it upon the precise point you wish, fairly and without getting entangled in it. In passing so big a caustic along the nostril the parts would be cruelly excoriated, were we not careful of the canal, which I do by cutting a stripe of sheep's leather, and conveying it high into the nostril with the probe, and, laying it flat and smooth along the surface to be cauterised; I leave it there, and turning the caustic towards it, I run it up to the point I design to burn. Upon the slightest sense of disappointment, I withdraw both, and begin anew; but being conscious that I have succeeded, I withdraw the sheath of leather at the same moment that I push up the caustic to the part, and I impress the caustic very firmly upon the part, for the instant it touches the naked surface, the eyes fill with tears, the patient draws a long breath, and sneezes tremendously, and instantly displaces it; but if you press firmly, this first irritation goes off: if you have passed it far beyond the strait of the nostril, and up to that point where always I conceive the roots of the tumor to lie,* it seldom is driven away by any future paroxysm of sneezing. It does sometimes happen that the profuse secretion of mucus carries it down, and the operation being performed at ten o'clock, for example, the caustic is discharged by two or three o'clock; but often I have found the caustic in both nostrils the next day at dressing. It will add to your assurance and confidence when I tell you, that deep as you may appear to yourself to have introduced the caustic, rather beyond the nostril as you would imagine, and on the very verge of its posterior opening, just over the palate, it never falls backward into the throat, nor ever is swallowed; of the many hundred times I have used the caustic, no such accident has ever happened.

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* The size we suppoſe must be proportionate to the space the polypus occupied. Mr. B. refers to a plate which is left out of our copy. *Ed.*

"The caustic," he continues, "I apply every second or third day; and I often continue this severe process during a whole month, with occasional intermissions;" previously wiping the nostril perfectly clean, with a large iron probe well armed with lint. The event he declares to be almost uniformly successful.

But though it is so difficult to noose an incipient or nasal polypus, our author positively asserts, that every polypus extending downwards so as to become guttural, may be noosed, "and that no person of common dexterity can fail to perform it." For this purpose he prefers silver wire about the size of harpsichord wire. The manner in which it is to be done is as follows:

"*First. How to pass the wire through the nostril to the throat.*—You have already felt the tumor depressing the palate, and estimated its size; you have repeatedly placed the patient before you, and made him open his throat, and depressed his tongue so as to occasion straining and retching, which unfolds all the parts, and shows you their size and relations; you now place him before you, opposite to a light, supported by assistants, resolute to bear whatever you do, and reconciled to it, by seeing that you have only a piece of silver wire in your hand; you take the piece of wire, about three feet long, double it, and smooth and arrange the double part, by pressing and modelling it in your fingers into a neat noose, a little open, and ready to expand when it gets into the throat, but small enough to pass through the nostril, and taking this loop betwixt your fore finger and thumb, you enter it into the nostril, and push it gently along. However big the polypus, you find the loop of silver wire glides easily and smoothly along; find it sometimes stopped, and then it bends and resists; but withdraw it a little, and then push it, and it will go on. I have never found occasion to use any instrument for conveying the loop to the throat, except when the bones were destroyed, a case in which I can hardly counsel you to attempt the cure." Should an instrument be required, a catheter, cut open so as to be converted into a canula, will answer the purpose of pushing the loop into the throat.

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* Certainly Mr. B. would not deposit his caustic upon supposition merely, but would ascertain the part to which it was to be applied. *Ed.*

When the loop has passed into the fauces you can easily see it, and taking hold of it with a hook, or a pair of dressing forceps, you pull it out of the mouth with one hand, and push it into the nostril with the other: this is a short but painful struggle, until you have drawn the loop without the mouth. "Often you will find the loop of your wire passing actually down into the glottis; the patient instantly cries, coughs, and strains violently, while the face becomes turgid, and the eyes stand in tears; instantly knowing what kind of an accident has happened, you withdraw the wire a little towards the nostril; by this motion you retract it from within the glottis, and you keep it carefully there until the straining ceases; then you push it gently on again, keeping the mouth open, and catching the wire the moment it appears behind the tonsils. In this part of the operation there is much address and some practice necessary; first, in stealing the wire on so gently that the patient scarcely feels it; second, in diving keenly and resolutely with the finger into the fauces the moment the wire begins to excite the throat, or becomes visible behind the velum; and, lastly, in quieting and composing the patient for the next part of the operation, viz. that of casting the noose, which is to be done in the following manner: you now draw out the loop of wire entirely from the mouth, and spread it wide; you prepare to react with the right hand in favour of the left; you take the two ends of the wire which project from the nostril firm in the left hand, twisting it around the fingers for a surer hold; you gather the fingers of the right hand together with the thumb so as to form a cone, and taking the loop of the wire upon the conical fingers, you, by drawing the wire up towards the nostrils, tighten the loop so upon the fingers, that it is not easily displaced in the next step of the operation; you now prepare for that effort by which you are to carry the loop over the biggest and most pendulous part of the tumor. In this effort you are to succeed or to fail; it can last but for a moment; the patient, while you make this effort, cannot breathe; he feels the severe pressure of your fingers in his throat; he is suffocating, struggling at once for breath, and striving to vomit; his eyes are staring, and his visage inflated; you dare not keep him one moment in this condition; you must act resolutely and dextrously. Your purpose is to push the ligature home over the bulk and body of the tumor with your right hand, while with the left you draw the ligature backwards towards the nose: you first allow the patient time to take breath and be composed; you let him fairly understand what you intend to do and how; you prepare yourself by making the wire tense, by pulling with the left hand, and fix the loop by spreading and distending a little the conical fingers of your right: you then, in one moment, retract the ligature steadily but speedily with the left hand, while you plunge the loop into the mouth, and carry it quite to the back of the throat with the right." Now, pulling the tumor towards you with the points of your fingers considerably bent, you unhitch the wire from them, and " at once turn it over the lower part of the tumor with the right hand, and pull the noose up towards the root of the tumor with the left."

" *How to hitch up the ligature close to the root.* With every operator this must be a matter of great anxiety, for he has no sign nor mark by which to know that the loop is carried to the highest possible point, nor any sure means of doing so.

" I have often found, especially in firm and smooth polypi, that I have, by that quick and forcible retraction of the ligature, by which I draw it up behind the polypus, hitched it at once so high that no after operation was either necessary or useful. But the attempt to hitch the ligature high, and place it correctly round the neck of the tumor, can never be superfluous. The instrument I most frequently use, is a boy's catheter, or one of the smallest size, cut across about the middle or somewhat short of the middle of its curve. Taking one of the ends of the wire as it hangs out of the nostril, I pass it through the tube of the catheter, and then holding both ends of the wire or noose firm, I pass the catheter deep into the nostril along the wire, till I imagine the point of the catheter touches the tumor; then by tightening both ends of the wire, and turning the point of the catheter upwards, I try to raise that side of the wire or ligature as high as possible; I then withdraw the catheter, pass the opposite side of the wire through it, I hold all tight again, and try to raise that side of the noose as high as possible; I then pass both wires through the tube of the catheter at once, push the catheter along till it touches the tumor, pull both ends of the ligature so as to tighten it round the neck or smaller part of the polypus, and, twisting the wire fast round the handle of the catheter, I leave it there. More frequently I use, for this latter purpose, a smaller tube, or very small section of a catheter, a little bended, which, when fixed, projects no more than an inch, or an inch and a half beyond the nostril, and is less apt to be discomposed by accidents during the day, or change of posture while the patient lies asleep.

"Sometimes I have run along the line of the ligature, to hitch it higher, a probe with the point bent. Sometimes, giving the loop a twist in its middle, I have, before introducing it through the nostril into the throat, tied a ligature of waxed thread, or catgut, which twist keeps what may be called the regulating ligature in the centre of the loop, so as to fix it to its proper place; when a ligature thus mounted is introduced into the nose, and the loop caught in the throat, and retracted through the mouth, this assistant ligature is fixed on the centre of it; the noose of wire is then carried into the throat upon the conical fingers of the right hand, and cast over the tumor, and retracted behind it, as I have already explained; and, of the three ends hanging out of the nostril, you raise first this supplementary ligature, by running the catheter along it, and thus you make sure of hitching the centre of the loop of silver wire higher; after which you carry each of the sides higher, by running the tube along them, and if you know the windings of the passages, and have formed a true conception of the form of the polypus you have to deal with, your chance is tolerable of placing the ligature very true."

These are the directions which Mr. B. gives for noosing polypi. He adds, that it will sometimes be requisite to tighten the ligature after a day or two, in consequence of the yielding of the tumor. Sometimes the polypus is swallowed and passed undigested; it is always separated in the course of six or eight days, when the wire may be withdrawn, if it has not previously fallen out. He subjoins several cases in which this mode of treatment was successfully practised.

His next "Discourse" is "on tumors of the gums, lips, cheeks, and throat." The following case contains a description of that cancerous affection to which the gums are liable, and the operation necessary for its cure.

"A young gentleman, Mr. H——, about twenty-five years of age, of an athletic form, and healthy constitution, and without the slightest taint of disease hereditary or acquired, had, from no perceptible cause, a tumor, firm, cartilaginous, and elastic, seated so fairly in the centre of the gums as to raise the two centre teeth of the lower jaw from their sockets far above the general range of the teeth, and separate the two that lay adjacent. Gradually, but yet in a short period, in a little more than three months, it had separated those two central teeth, projected from betwixt them, and increased to a very remarkable size; and, though it had begun from the inside gum, the greater portion seemed to be before the gum just over the chin projecting the nether lip.

"This tumor was about the size of a walnut, irregularly globular, knobby, and shining; when moved by pressing it with the thumbs backwards and forwards, it seemed firm, fixed, and actually a tumor of the jaw-bone; yet I knew it by its smooth *knobular* form, and its elasticity, to be a tumor of the gum. It felt, when pressed between the finger and thumb, so elastic that a tyro must have believed it to contain a fluid, while I knew it to be, in truth, a solid tumor, neither suppurated nor capable of suppuration. Its general aspect was that of a tumor so indolent that nothing need be dreaded from it.

"Dr. Munro, Mr. Allen and myself, having consulted on the nature of this tumor, judging by the analogy of former cases, declared it to be of a most dangerous nature; it had already displaced the teeth, probably injured the alveolar process of the jaw-bone, attained to an alarming size, and threatened in no long period of time to assume the most malignant form: we explained to our patient that it contained no matter, was incapable of suppuration, was of a size that precluded all hopes of resolution, was sure to become cancerous, admitted of no delay; and we explained that we dared not even in its first and least alarming stage, do less than extirpate it from the very root: we represented that the operation was harsh, but not tedious;

void of danger, even of hemorrhage: that nothing was to be feared but the not cutting it completely out.

"In performing this operation, I held the jaw-bone firm with the fingers and thumb of my left hand, while my assistant inverted the lower lip, and, with a scalpel of special strength, broad backed like a cartilage knife, I made an incision by the side of the displaced teeth in such a form as to represent the letter V, or the Greek delta. All my strength of hand was requisite to carry the knife down to the angle; blood instantly filled the mouth, so that, after the first stroke of the scalpel, every thing was done by feeling; but I cut with such decision, with such level lines, and made them meet so fairly at the angle, that, by pressing my two thumbs, one within side the tumor, the other without, and pushing alternately with my thumbs, and poising with the flat handle of the scalpel, I pushed the tumor out clean and unmangled, leaving a very wide opening, bounded by the fangs of the adjacent teeth; and, after allowing the part to bleed till it stopped, I laid a piece of sponge in the deep triangular cleft made by the operation, and closing the upper range of teeth upon the sponge, their pressure kept it in its place. During eight or ten days our patient eat cautiously, and in little more than ten days the incision was healed."

There is nothing remarkable in the account he gives of the tumors of the "lips, cheeks, and throat," nor in what he says concerning salivary tumors, except the singular boldness of his operations for their removal. The following is selected to exhibit this in the strongest light:

"Jane Sharp, aged 27. January 15th.—About twelve years ago, without any evident cause, she observed a small, very painful tumor, forming immediately under the right ear, which gradually increased for four years, until it had arrived to the size of a large goose's egg, rising up anterior to the ear. At this time she had it cut out: during the operation she lost a considerable quantity of blood, and suffered great pain; since which she has been unable to move the muscles of that side of the face, which is paralyzed in consequence of the division of the portio dura, or facial nerve. About a year after the tumor began again to form, accompanied occasionally by lancing pains; since which time it has increased gradually. It is at present about the size of a large hen's egg, hard, unequal on its surface, quite colourless, and at times accompanied by stinging pains. It extends considerably behind the jaw, and on the anterior part of it is seen the cicatrix from the former operation.

"17th. A tolerable night's rest, no hemorrhagy, but she complains of pain in the throat and difficult deglutition.

"18th. She complains to-day of difficulty of breathing, the pain of the throat continues; the bandage being slackened, there is immediate relief to both; she sleeps ill, opium is substituted for the draught.

"19th. The sponges adhere firmly to the surface of the wound; there is little tension or inflammation of the parts, but she complains of the pain in the throat, chiefly on deglutition.

"20th. Two of the sponges were removed with little difficulty or pain, no hemorrhagy, the wound is filled up with lint.

"22d. All the sponge is removed.

"25th. The discharge is copious.

"February 4th. The cavity fills up rapidly.

"15th. She continues to do well.

"25th. She was dismissed nearly well."

Report of the operation by Mr. Bell, January 16:

"The one half of the parotid gland, that which lies behind the angle of the jaw, had been left: the scab of the former operation was on the fore part of the tumor: the tumor lay behind and under the ear, pressing deep betwixt the mastoid process and the rising branch of the jaw bone. It seemed outwardly a small tumor, but it had shot down very deep, adhering to the pterygoid process, and to the back of the pharynx. The dangers of the operation were these: the portio dura, the ninth pair of nerves, passed through the tumor: immediately under it lay the trunk of the carotid. The external carotid behind the lower jaw, but chiefly the large jugular vein, was in danger, for the tumor lay so entirely under the mastoid muscle, that part of the muscle is taken away with the gland. The gland being dissected free in its lower part, where it lay near the carotid artery, was torn up by the roots where it adhered to the back of the fauces. The internal maxillary artery bled very profusely; a small piece of sponge, the size of the thumb, and supported by other pieces of sponge, filling up the hollow, was pressed upon the artery at the back of the nostril. The swelling of this piece of sponge caused a difficulty of deglutition, which ceased when the sponge was removed. After the operation I put my finger into the hollow whence the gland was extracted, which I felt two inches and a half deep. At its lower angle, (i. e.) behind the corner of the jaw-bone, the carotid lay bare, beating strongly, not dilated; the upper part of the wound was deep, so that the finger touched the pterygoid process forwards, and the apophysis cuniformis of the occiput backwards; and when she swallowed, the morsel, in passing down the pharynx, pressed upon the point of the finger."

Page 210 Mr. B has the following observation. "For I had often extirpated the diseased parotid;" and, on the next page, says it cannot be done without cutting the carotid, which he admits elsewhere must be fatal. This is one of those errors, which a more plodding genius would have avoided. He likewise mentions a fatal case of aneurism of the carotid; and an instance is related of the tic doloreux cured by dividing the nerve. We believe, however, that this operation has rarely succeeded.

The work is concluded by a "Memoir on the Surgical Diseases of the Poor," which, though highly creditable to the author, happily does not apply to this country.

The copious extracts which we have made from the work before us fully evince our opinion of its merits; and we sincerely lament that an edition of the whole of Mr. B.'s Principles of Surgery cannot be printed here to advantage; even an abridgment of them will, we think, if well executed, be of great benefit to the profession. There is one announced by Mr. Smith, whose prospectus will be found under the head of Intelligence, which, it is expected, will appear in the course of the next autumn.

Memoirs of the Philadelphia Society for promoting Agriculture. Containing Communications on various Subjects in Husbandry and rural Affairs. To which is added a Statistical Account of the Schuylkill permanent Bridge. Philadelphia. Jane Aitken. 1808. *Vol.* i. 8vo. pp. 479.

WHEN nearly all the information we have on any particular art or science is contained in two or three books only, these books, whatever may be their real merit, derive from that circumstance alone a degree of intrinsic importance, which they would not otherwise possess: So it is with respect to publications on American husbandry. They are so few in number, that they are entitled, on that account, to a much better reception than they would otherwise meet. If we have but a small portion of written information on this subject, it will be remembered, that it is our little all, and must be estimated accordingly. In a country like ours indeed, which depends chiefly for its wealth and prosperity on the productions of the soil; where correct agricultural knowledge is by no means generally diffused; where the methods of managing farms are learnt by rote, without system and without science; where the husbandman generally pursues, with undeviating steps, the path of his ancestors;—in such a country every species of information on agriculture must be considered highly important, and cannot fail to be advantageous to the community. We have then no hesitation in affirming that the volume before us may be ranked amongst the most useful American publications: for although it contains many things, which in other countries might not be thought worth printing, yet even these may be beneficial here, because they are new to a large proportion of our farmers. With a view, therefore, to encourage the general diffusion of agricultural knowledge, we are inclined to bestow every possible commendation on works of this kind, and especially on those which relate to practical husbandry. They ought to be multiplied to a much greater extent than they have been, and no farmer ought to be without such a book for his guide. He may always procure some valuable knowledge from this source, and thereby enrich the stock of his own experience.

The establishment of societies for the purpose of collecting and distributing information on American husbandry, is, perhaps, the best method of accomplishing that important object. Such societies ought to be instituted in every state in the union, and, if possible, in every county of the several states. Pennsylvania, Massachusetts, and New-York have set a laudable example in commencing these institutions. The societies established in those states have already furnished us with some of the most valuable communications on agriculture, as well as on arts and manufactures. They are generally composed of men of science residing in the cities where the meetings are held, and also of practical intelligent farmers scattered over the country. In consequence of this organization they are well calculated to attain the objects contemplated by them. But to render these institutions still more beneficial than they otherwise would be, it has been a favourite object in New-York and in Pennsylvania to establish and to connect secondary societies throughout these respective states. The New-York society, which meets at the seat of government, at Albany, during the session of the Legislature, several years ago appointed secretaries residing in the several counties, with instructions to endeavour to form county societies, which might correspond with the general state society, and send their communications to it for publication. But the success of this measure was by no means equal to what might have been expected. Notwithstanding all the exertions of the society, their plan was not carried far into execution. In the state of Pennsylvania too, we see, with pleasure, that a similar project was long ago formed. But, unfortunately, this also failed in its accomplishment. At some future period, perhaps, when the value of such institutions shall be more justly appreciated, and consequently more importance attached to them, these experiments may be repeated with success. We shall then soon be in possession of all the agricultural knowledge in our country; and every farmer will be enabled to avail himself of the experience of every other farmer concerning the best system of husbandry adapted to the peculiar soil, climate, and condition of these United States. The great mass of information on English husbandry, found in the county reports, made to the Board of Agriculture of Great-Britain, demonstrates both the utility of this kind of information, and the practicability of obtaining it, whenever the people shall be sufficiently convinced of its importance. But with us this period, we fear, is not yet arrived. In the mean time, it becomes us to acquaint our readers with the correct and enlightened views of the Philadelphia Society on this subject, by giving their plan for establishing a state society, as contained in the introduction to their Memoirs. It is as follows:

"1. The legislature to be applied to for an act of incorporation of the society, which is to consist of citizens of the state, as generally dispersed throughout the same as possible. In the first instance, the society to be composed of such persons as may be named, and these to be vested with authority to make rules for admission of other members, and bye-laws for the government of the society, as usual in similar cases. Honorary members to be admitted according to rules to be established, and these may be of any state or country.

"2. The organization of the society shall be so formed, that the business thereof may be done by a few, who will be responsible to the body of the society, in such manner as their bye-laws shall direct.

"3. The governor of the state, the speakers of the houses of the legislature, and the chief justice, for the time being, to be the visitors of the corporation. The transactions of the active members, i. e. those entrusted with the monies and affairs of the society, by whatever name or description they may be designated, and all bye-laws and regulations, to be submitted to the visitors; to the end that the same may be so conducted and established as not to prejudice the interests of the corporation, or interfere with or oppose the constitution or laws of the state. The visitors will also judge of the objects of the society, and perceive whether or not they are calculated to promote the ends of its institution. Reports may by them be made annually to the legislature. These will be useful, as they will exhibit in a comprehensive view, the state of agriculture throughout the commonwealth, and give an opportunity to the legislature of being informed on a subject so important to the prosperity of the country, both as it relates to political œconomy, and the individual happiness of the people. The legislature will perceive from their reports, when, and in what manner, they may lend their assistance to forward this primary object: Whether by endowing professorships, to be annexed to the University of Pennsylvania and the College of Carlisle, and other seminaries of learning, for the purpose of teaching the chemical, philosophical, and elementary parts of the theory of agriculture: or by adding to the funds of the society, increase their ability to propagate a knowledge of the subject, and stimulate, by premiums and other incentives, the exertions of the agricultural citizens: or whether, by a combination of these means, the welfare of the state may be more effectually promoted.

"4. Though it will be most convenient to make the repository of the information of the society, and the office or place of transacting its business at Philadelphia; yet it is intended that the society shall be rendered active in every part of the state. To effect this, there should be county societies established, organized as each shall think proper. In union with, or as parts thereof, there may be agricultural meetings or establishments, at the will of those who compose them, in one or more townships of a county. These may correspond with the county societies, and the latter may annually inform the society of the state (of which the less societies may be considered as branches) of all the material transactions of their respective societies. Societies already formed may remain as they are. They may, at their option, correspond directly with the state society, or through the society of the county in which they meet, as shall be found most convenient and agreeable to them. They will thus collect all the information and business relating to the subject, and will give an opportunity to the society of the state to see where their assistance is most necessary, and afford a facility of diffusing agricultural knowledge. The premiums, books, and other articles at the disposal of the society, may pass through the hands of the county or other societies, for many purposes; and they can judge on the spot of the pretensions of the claimants. The county school-masters may be secretaries of the county societies, and the school houses the places of meeting, and the repositories of their transactions, models, &c. The legislature may enjoin on these school-masters, the combination of the subject of agriculture with the other parts of education. This may be easily effected, by introducing as school books, those on this subject, and thereby making it familiar to their pupils. These will be gaining a knowledge of the business they are destined to follow, while they are taught the elementary parts of their education. Books thus profitable to them in the common affairs of life, may be substituted for some of those now used; and they can easily be obtained. Selections from the best writers on husbandry may be made by the society. The essays of our own experimentalists or theorists, and the proceedings of the society, will also afford information; and, as many of these will, no doubt, be good models of composition, they may form a part of the selection for the use of the county schools. And thus the youth in our country will effectually, and at a cheap rate, be grounded in the knowledge of this important subject. They will be easily inspired with a thirst for inquiry and experiment, and either never acquire, or soon banish attachments to bad systems, originating in the ignorance and bigotry of their forefathers, which, in all countries, have been the bane of good husbandry. It will also be the business of the society to recommend the collection of useful books on agriculture and rural affairs in every county. The citizens of the country should be drawn into a spirit of inquiry by the establishment of small, but well chosen libraries, on various subjects. This would not only promote the interests of agriculture, but it would diffuse knowledge among the people, and assist good government, which is never in danger while a free people are well informed.

" 5. The general meetings of this society, consisting of such members as may choose to attend, and particularly those charged with communications or information from the county and other societies, should be held at Philadelphia, at a time, in the winter sessions of the legislature, when citizens who may be members thereof, or have other business, can with most convenience attend. At these meetings, the general business of the society can be arranged, its funds and transactions examined, and its laws and rules reported, discussed, and rendered generally serviceable and agreeable to the whole.

" 6. It will be necessary that a contribution be made by each member, annually, for a fund. But this should be small, that it may not be too heavy a tax. The funds will, no doubt, be increased by donations from individuals; and if the state should find the institution as useful as it is contemplated to be, the patriotism of the members of the government will be exercised, by affording assistance out of the monies of the state. They will perceive that it is vain to give facility to transportation, unless the products of the country are increased by good husbandry: And though these facilities are important to the objects of this society, yet an increased knowledge of agriculture is the foundation of their extensive utility. The subjects of both are intimately connected, and mutually depend on each other.

"7. When the funds of the society increase sufficiently to embrace the object, it will perfect all its efforts by establishing *Pattern Farms*, in different and convenient parts of the state. Let the beginning of this plan be with one establishment, under the direction of the society, and committed to the care of a complete farmer and gardener. In this, all foreign and domestic trees, shrubs, plants, seeds or grains may be cultivated, and if approved as useful, disseminated, with directions for their culture, through the state. The most approved implements may be used on this farm, and either improved by additions, or simplefied to advantage. Inventions may be brought to trial, and the best selected. Models thereof may be made and transmitted to the county and other societies. Those who are sent to, or occasionally visit the farm, will gain more knowledge, in all its operations, from a short inspection, than can be acquired, in a long time, by reading on the use and construction of instruments, or the modes of cultivation. The cheapest, best and most commodious style of rural architecture—the most proper and permanent live-fences—improvements in the breed of horses, cattle and sheep—remedies for occasional and unforeseen visitations of vermin—the times and seasons for sowing particular crops—the adapting foreign products to our climate—and preventives against all the evils attendant on our local situation, or arising from accidental causes—may here be practically introduced. The thoughts and suggestions of ingenious men may here be put in practice; and, being brought to the test of experiment, their utility may be proved, or their fallacy detected. This farm need not be large. On it the best systems now known may be carried through, and farther experiments made: promising youths may be sent from different parts of the state to learn practically the arts of husbandry. Manures, and the best mode of collecting them, may be tried; native manures should be sought after, and premiums given for their discovery. Their efficacy may be proved by small experiments on this farm, which should, in epitome, embrace the whole circle of practical husbandry. Similar farms may be added, as the funds increase, and thus practical agricultural schools be instituted throughout the state.

"8. When the pecuniary affairs of the society become adequate, it will highly contribute to the interest of agriculture, if, at the expense of the society, some ingenious person or persons were sent to Europe for the purposes of agricultural inquiries. It would be well too, if a few young persons of promising abilities were sent thither to be instructed in the arts of husbandry, the breeding of cattle, &c. and to gain a practical knowledge on all subjects connected with this interesting, delightful, and important business, on which the existence, wealth, and permanent prosperity of our country so materially depend.

"9. Although it would seem that a great portion of this plan has reference to the older settlements of the state, yet, in fact, many of its most useful arrangements will apply to new settlements in an eminent degree. These settlements are, for the most part, first established by people little acquainted with a good style of husbandry. The earth, in its prime, throws up abundant vegetation, and for a short period rewards the most careless husbandman. Fertility is antecedent to his efforts; and he has it not to re-create by artificial means. But he is ignorant of the most beneficial modes whereby he can take advantage of this youthful vigour with which his soil is blessed. He wastes its strength, and suffers its riches to flee away. A bad style of cropping increases the tendency of fresh lands to throw up weeds, and other noxious herbage; and that luxuriance, which with care and system might be perpetuated, is indulged in its own destruction. It is discovered, when it is too late, that what was the foundation of the support and wealth of the improvident possessor, has been, by his ignorance and neglect, like the patrimony of a spendthrift, permitted, and even stimulated rapidly to pass from him in wild extravagance.

"The products of nature, in our new countries, seldom have been turned to account. The timber is deemed an incumbrance, and at present is, perhaps, too much so. The labour and expense of preparing for tillage are enormous; and, when the sole object is that of cultivation, very discouraging.* European books give us no lessons in these operations. But when the experience of our people is aided and brought to a point, by an union of facts and the ingenuity of intelligent men, now too much dispersed to be drawn into system, it is to be expected, with the surest prospects of success, that our difficulties on this head will be abated, if not overcome. The manufacture of potash, and the products of the sugar-maple, may be objects of the attention of the society. More profitable modes of applying labour will hereby be promoted, and returns for expense, in the preparation for culture, be obtained. Facilities for clearing lands may be discovered. Minerals, earths, and fossils, now either unknown or neglected, may be brought into use, or become objects of commerce. In fine, no adequate calculation can be formed of the effects which may be produced by a consolidation of the efforts, and even speculations, of our citizens, whose interests will stimulate them to exertion. Channels of communication will be established, and the whole will receive the benefits arising from a collection of the thoughts and labours of individuals, whose minds will be turned to a subject so engaging and profitable, as well to themselves as to their country."

After this long extract, we have no room to notice particularly the variety of agricultural information contained in the

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"* At the present time (1808) the expense of clearing land is much lessened, owing to the great influx of population in our new countries; for five dollars per acre, land may be completely cleared of timber.

body of this volume. Our readers must be referred for that to the work itself.

The appendix contains an elaborate description, and a drawing of the Schuylkill bridge. This is a useful document in the art of bridge building. The invention of the cover to preserve the work from decay, we think a most valuable improvement. The whole of the structure is indeed a credit and an ornament to our country. The projectors and directors of it are entitled to great merit, and their persevering exertions ought to excite others to "*go and do likewise.*"

INTELLIGENCE.

DOMESTIC.

Medical School of New-York.

THIS institution is under the immediate direction of the Regents of the University of the State. The Professors are appointed by them, with the style and title of Professors of the University, and their lectures are delivered in the College of Physicians and Surgeons. It is made the duty of the College, by their charter received from the Regents, to procure the necessary buildings and accommodations for the professors and students, to provide an anatomical museum, a chemical laboratory and apparatus, a library, botanic garden, &c. The Professors of the University, who are also trustees and members of the College, together with the President, Vice-Presidents, Register, and Treasurer of the College, constitute a Senatus Academicus, who direct the system of education to be pursued, and make all necessary rules for the government of themselves and of the students. The Regents are appointed by the Legislature of the State in the same manner that Senators in the Congress of the United States are appointed. They are usually selected from amongst the most prominent characters in the state, and have the general superintendence of all the colleges and academies in it; their names are as follow:

Regents of the University.

His Excellency the Governor, (ex officio) *Chancellor.*
The Lieutenant Governor, (ex officio) *Vice-Chancellor.*
Rev. John Rodgers, D. D.
Ezra L'Hommedieu,
Nathan Carr,
Matthew Clarkson,

Ambrose Spencer,
Henry Rutgers,
Andrew King,
Simeon De Witt,
John Tayler,
Ebenezer Russel.
James Cochran,
James Kent,
Elisha Jenkins,
De Witt Clinton,
Charles Selden,
Peter Gansevoort,
Abraham Van Vechten,
Alexander Sheldon.

Professors of the University.

Nicholas Romayne, *M. D. Professor of the Institutes of Medicine.*

Samuel L. Mitchill, *M. D. Professor of Botany and Natural History.*

Edward Miller, *M. D. Professor of the Practice of Physic.*

Archibald Bruce, *M. D. Professor of Mineralogy.*

William J. M'Neven, *M. D. Professor of Obstetrics.*

J. Augustine Smith, *Professor of Anatomy and Surgery.*

Benjamin De Witt, *M. D. Professor of Chemistry.*

To support this Medical School, the Legislature have lately granted the sum of $20,000 to the Regents of the University, for the endowment of the College. They have also made provision by law for procuring subjects for dissection. The number of gentlemen attending the lectures of the Professors the present year are seventy-two, and will, no doubt, increase in proportion as the advantages of studying medicine in this institution shall become more known throughout the United States.

New-York Hospital.

The Governors of the New-York Hospital have lately appointed Dr. John C. Osborn and Dr. Benjamin De Witt Physicians to that Institution.

New-York Historical Society.

At an election of officers of the New-York Historical Society, held at the City-Hall, 10th January, 1809, the following persons were chosen:

Egbert Benson, *President.*
Right Rev. Bishop Moore, }
Brockholst Livingston, } *Vice Presidents.*
Rev. Samuel Miller, *Corresponding Secretary.*
John Pintard, *Recording Secretary.*
Charles Wilkes, *Treasurer.*
John Forbes, *Librarian.*

Standing Committee.

William Johnson,
Samuel L. Mitchill,
David Hosack,
Anthony Bleecker,
Rev. Dr. John M. Mason.
De Witt Clinton,
John M'Kesson.

American Mineralogy.

Dr. Adam Seybert, of Philadelphia, has commenced publishing in the Philadelphia Medical Museum, a catalogue of American minerals. This is an undertaking honourable to Dr. Seybert, and likely to prove useful to our country.

Decomposition of the Alkalies.

By a letter from Edinburgh we are informed that Dr. Hope and Mr. Murray have repeated the experiments of Mr. Davy on the alkalies, and have succeeded in procuring the metallic globules. The discoveries of Mr. Davy, by means of galvanic electricity, have induced Mr. Murray to alter the arrangement of part of his System of Chemistry, a new edition of which he is now preparing for the press.—Mr. Playfair is also preparing for the press a new edition of his Illustrations of the Huttonian Theory.

Prospectus of a Surgical Work.

The want of a new System of Surgery must be sensibly felt by the great majority of American Surgeons. John Bell's, the last, and by far the most valuable work in this department of medicine, is so costly as to be read by a small proportion only of the profession in this country. To remedy this inconvenience, application has been made to Mr. JOHN AUGUSTINE SMITH, of the Royal College of Surgeons in London, and Professor of Anatomy and Surgery in the College of Physicians and Surgeons of New-York, to publish an abridged edition of the "*Principles of Surgery.*" But to reduce Mr. Bell's book to any moderate size, to correct where correction might be deemed necessary, to compare him with other authors, and to state the result, would be so to change the work that it could no longer be called BELL'S SURGERY. Moreover, it is certainly improper to publish mutilated copies of a work, particularly during the life of the author. Such an opinion is entertained of the merit of Mr. Bell's performance, that if a cheap edition of it could have been printed here, it would not have been necessary to undertake a new system of Surgery. The work is intended to be so nearly an abridgment of Mr. Bell's, as to supersede the necessity of such a publication, and, at the same time, so much an original performance as to admit of the introduction of any important matter from other sources. It will be published by Messrs. Collins and Perkins.

They have likewise in the press the *Anatomy of the Human Body*, by JOHN and CHARLES BELL, in two large octavo volumes, from the third London edition corrected, and illustrated by one hundred and twenty-five engravings.

The publication in this country of medical works, at all times a matter of the greatest consequence to the profession, is now peculiarly so, when the importation of foreign books is entirely suspended. The cheapness of American editions, for they are sold at half, or less than half the price charged for those printed in London, is a consideration of great importance, particularly to those who purchase to a considerable amount. It has been objected to the editions issued from our presses, particularly those in which there are plates, that though cheaper, they are likewise inferior to those imported. This we think will not be the case with the above edition of Bell's Anatomy, for we have seen some of the plates which are superior to those in the London copy. The extensive sale of the book will, we hope, remunerate the activity and enterprise of the publishers.

Quincy's Lexicon improved.

Messrs. T. & J. Swords, the proprietors of the improved American edition of *Quincy's Lexicon*, are preparing for the press a new edition of this valuable medical dictionary, which it is expected will make its appearance in the course of the ensuing summer.

FOREIGN.

Account of farther Discoveries in extracting Metals from the Earths and Alkalies.

The Swedish chemists, M. Pontin and Berzelius, have succeeded in obtaining amalgams of the metals of lime and barytes, of magnesia and of strontites, by exposing these earths moistened to negatively electrified mercury; but this method has no effect on alumen or silex. These gentlemen have also made a very interesting experiment on ammonia; they placed a solution of it in contact with mercury, negatively electrified, as in the former experiments, and thus succeeded in obtaining a soft amalgam, which proves its metallic nature.

Mr. Davy has since improved on the discovery of the Swedish chemists. The amalgam of barytes and mercury obtained by their method was distilled, and the mercury being driven off, he obtained the metal of barytes in a pure form: and by the same process he procured the metals of strontites and magnesia nearly pure. The earths are mixed with red precipitate, which is negatively electrified; the amalgam is absorbed by fresh mercury, and when it becomes semi-fluid, is distilled in the vapour of naphtha in a tube of plate glass.

The above facts were communicated by Mr. DAVY to the Royal Society a few weeks ago, when he also stated, that in experiments made with a voltaic battery of 36,000 square inches, on barytes, strontites, and the other alkaline earths, and on silex and alumen; all these bodies, when slightly moistened, and acted on by iron wires negatively electrified, undergo changes at the points of contact; and the metals of the earths appear to form alloys with the negatively electrified iron.

Mr. DAVY likewise metallized the earths by electrifying them when mixed with the oxyds of lead, of silver, or of mercury, when the metals of the earths and the common metals were revived together in the state of alloy.

Since Mr. DAVY's first discovery of the metallic nature of the alkalies by the voltaic battery in last November, several others have been made relative to them and the earths, with a rapidity seldom witnessed before on similar occasions. Soon afterwards the discovery of the French chemists was made, that the metal of potash might be obtained in large quantities without the aid of galvanism, by means of the superior attractions of iron and charcoal for oxygen in a strong heat, and the tendency of the new metal to form an alloy with iron; and in the foregoing short notice are contained no less than four more important discoveries; that of the Swedish chemists, of the effect of negatively electrified mercury in accelerating the metallization of the earths; and those of Mr. Davy, of the obtaining these metals pure, by the distillation of the amalgams so formed; of the operation of the oxyds of lead, silver, and mercury, when electrified, in reviving the metals of the earths by forming alloys with them, and of the same effect produced by negatively electrified iron in the metallic state.

The discovery of ammonia being of a metallic nature, shows that it is probable the very air we breathe contains metal in a gaseous state; for azote, which forms a large portion of our atmosphere, has long since been proved to be a component part of ammonia.

Silex and alumen having both proved refractory to the processes which obtained metals from the other earths, gives additional weight to the old opinions of their being of the same nature, or of their being different modifications of the same substance. {*Athenæum, Sept.* 1808.

Davy and Singer's Chemical Experiments.

It has been asserted by most writers and experimentalists, that silver burns with a bright emerald-green light. In Mr. DAVY's late lectures at the Royal Institution, the deflagration of silver leaf was attended by the emission of a brilliant white light, which the professor ascribed to the great purity of the silver employed; and he expressed an opinion that the green flame usually observed, arose from the admixture of copper with the silver. Mr. G. B. SINGER has, however, discovered that this phenomenon proceeds from a different cause. Having observed that Mr. Davy's conducting wires were terminated by charcoal, he repeated the experiment with a similar arrangement; and applying the charcoal to a pure silver leaf, it immediately burned with a beautiful white light. Some of the same portion of silver having been before employed when the green flame was produced, it became evident that the white light in this and in Mr. Davy's experiment proceeded from the charcoal; and that this was really the case, appeared from the immediate evolution of green light, when the contact was made by a metallic wire. By the application of charcoal to the extremity of a wire, so bent that either the wire or the charcoal may touch the silver at pleasure, the white and the green flame may be alternately produced; and a conclusive demonstration of the fact, with a pleasing variation of a brilliant experiment, will be thus at once afforded.

Reade and Hincks's Chemical Experiments.

Dr. JOSEPH READE has published an account of an experiment, the result of which is in direct contradiction to the received opinion, that the agitation or friction of fluids cannot excite sensible heat. It is as follows:—The temperature of the apartment being 40°, half a pint of water at a similar heat, was poured into a tin, bottle-shaped vessel, into the aperture of which was inserted a thermometer surrounded with chamois leather, and made to fit accurately, with its bulb nearly in the axis. After briskly agitating the vessel for a few minutes, to his great surprise, he found that the temperature of the water rose eight degrees; and, even after the apparatus was uncovered and laid at rest on the table, the water continued to rise for several minutes; proving the origin of the heat to be inherent in the fluid, and independent of any external causes. Anxious to obviate every source of fallacy or objection, Dr. Reade prevented the communication of caloric by his hands, or of radiation from his body by coating the tin vessel with many layers of woollen cloth carefully wrapped round it, over which there was a tin case, the entire, nearly two inches in thickness, and covered externally with three wet towels. In the course of the experiment he dipped his hands frequently in snow water, and also sprinkled the towels. The Rev. Mr. Hincks, lecturer on chemistry in the Cork Institution, to whom the experiment was communicated, on repeating it in a glass bottle, found the heat of the vessel, by means of a thermometer placed between it and the covering, to be inferior to that of the enclosed fluid, and on a par with the atmosphere, which proves, in a most satisfactory manner, that there could be no communication of caloric from the hand.

Chemical Effects of Galvanic Electricity.

Some of the extraordinary effects of electricity in producing chemical decomposition, and in changing the ordinary affinities of different substances for one another, mentioned in Mr. DAVY's first Bakerian Lecture of 1806, are the following:

When compounds soluble in water were put into water contained in agate cups, and electrified, the decomposition was very rapid. A solution of sulphate of potash being put into each of the cups, and electrified by means of 50 pairs of plates for four hours, the acid was found by itself in the positive cup, and the alkaline basis in the negative cup. Similar phenomena took place with solutions of sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate, oxalate, and benzoate of ammonia, as also with alum. When muriatic salts were used, they yielded oxymuriatic acid. If compatible mixtures of neutro-saline compounds were used, the different acids and bases separated in a mixed state, without any regard to their affinities. When metallic solutions were employed, metallic crystals, or depositions, were formed on the negative wire, and oxyd was likewise deposited round it; at the same time a great excess of acid was soon found in the positive cup. This effect took place with solutions of iron, zinc and tin, as well as with the more oxydizable metals. When muriate of iron was used, there was deposited on the negative wire, a black magnetic substance, that was soluble with effervescence in muriatic acid; and when sulphate of zinc was used, a grey powder, possessed of the metallic lustre, and likewise soluble with effervescence, was deposited on the negative wire.

Although stronger solutions afforded signs of their decomposition quicker than weaker ones, yet even the most dilute solutions seemed to be acted upon with equal energy. Thus, paper tinged with turmeric, was immediately rendered brown when plunged even into pure water, and brought in contact with the negative point, in consequence of the very minute portion of saline matter contained in the paper. In the same manner, and from the same cause, paper tinged with litmus was immediately reddened by being brought in contact with the positive point. And in all these decompositions, the separation of the constituent parts from the last portions of the compounds, was complete when the operation was sufficiently protracted.

In the same manner, the acid, or alkaline elements of compounds could be transferred from one electric surface to the other. Pure water being electrified in cups of different substances, it was found that when the cup made of sulphate of lime was positive, and that of agate negative, the first contained sulphuric acid, and the other lime-water. On reversing the position of the wires, the agate cup contained sulphuric acid, and the other lime-water. Similar effects took place when pure water contained in one glass tube, and a neutral or metallic saline compound in another tube, were connected by moistened amianthus, and electrified. The acid always passed into the water when it was positive, and the alkali when it was negative: the metals and metallic oxyds passed like the alkalies, and collected round the negative point. When solution of nitrate of silver was positive, and water negative, the transmitting amianthus was covered with a thin metallic film. The time required for this transmission seemed, cæteris paribus, to be as the length of the intermediate volume of water.

The immediate contact of the solution to be decomposed with the wires, was not indispensably necessary for its decomposition. A solution of muriate of potash being put in the middle tube of a series of three tubes, the two outer tubes containing only water and the wires, alkali soon appeared in that connected with the negative wire, and acid in the other: and at length the acid and alkali were obtained perfectly separate.

In thus causing sulphuric acid to be transferred from sulphate of potash into water through moistened amianthus, no change was observed to take place, in paper tinged with litmus, placed near the amianthus. The reddening of the tinged paper always took place just above the point of the positive wire, and then slowly diffused itself to the middle of the tube, but no rediness appeared above the amianthus, or about it on the negative side. Similar effects were observed when the alkali was transferred, paper tinged with turmeric first becoming brown close to the negative wire.

When three glass tubes were used, the two outer tubes holding a solution of muriate of soda and the middle one sulphate of silver, paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the other, neither of the papers had any change made in their colours, although the muriatic acid passing through the amianthus occasioned a dense heavy precipitate in the sulphate of silver, and the soda a more diffuse and lighter one.

Acid or alkaline substances will also pass through liquids having a strong attraction for them. In an apparatus composed of three tubes, it was found that sulphuric acid, after being evolved from a solution of sulphate of potash, contained in the tube connected with the negative wire, would pass into water contained in the tube connected with the positive wire, through either ammonia, lime-water, or weak lixivia of potash, or of soda. The only effect of strong lixivia of potash, or of soda, was to increase the time necessary for this transfer. Muriatic acid, or nitric acid, were also transmitable in this manner through alkaline solutions; and *vice versâ*, alkaline matter, by changing the arrangement of the liquids, was transmitable in a similar manner through the acids: even magnesia was transferred like the other bases. But, however, when it was attempted to pass sulphuric acid through aqueous solutions of barytes, or of strontites; or, on the contrary, to transfer these earths through sulphuric acid, the sulphate of those earths was deposited in the intermediate tube.

Metallic substances required a longer time than the alkalies to pass through acid solutions. When a solution of green sulphate of iron was positive, muriatic acid intermediate, and water negative, green oxyd of iron began in ten hours to appear on the amianthus on the negative side, and in three days a considerable portion of this oxyd was deposited in the tube. Analogous results were obtained from solutions of sulphate of copper, nitrate of lead, and nitromuriate of tin.

Water being positive, a solution of sulphate of potash intermediate, and a solution of muriate of barytes negative; sulphuric acid, and afterwards muriatic acid were found in the water.

A solution of sulphate of potash being positive, a solution of muriate of barytes intermediate, and water negative; barytes first appeared in the water, and afterwards potash.

A solution of muriate of barytes being positive, a solution of sulphate of potash intermediate, and water negative; potash appeared in the water, and sulphate of barytes was deposited in the intermediate vessel, while no barytes passed over into the water, not even in ten hours.

Water being positive, a solution of sulphate of silver intermediate, and a solution of muriate of barytes negative; only sulphuric acid appeared in the water, and muriate of silver was deposited in the intermediate vessel.

Several of these experiments relating to the transference of acid and alkaline matter were tried with vegetable and animal substances. When a fresh leaf-stalk of polyanthus was the medium of communication between a solution of nitrate of strontites positive, and water negative, free nitric acid was found in the positive tube, and potash and lime passed immediately into the water; but it was not until after some time that strontites appeared in the negative tube. [*Retrospect.*

Knight's Description of a new Eudiometer.

Mr. R. Knight has published a description of a new Eudiometer lately made on the suggestion of Mr. Davy, for the more commodious display of the formation of water by the combustion of oxygen and hydrogen gas, by means of the electric spark. The instrument consists of a strong cylindrical glass tube to receive the gases, open at the lower end, of the capacity of two cubic inches, and graduated into decimal parts; and a stand to which the tube is attached by a clasp and screw, and of a strong iron cylinder, containing a strong spiral spring, on the principle of the pocket steelyard, the spindle or central bar of which is fixed on three feet, in order that it may be firmly secured on the side of a mercurial bath, with the mouth of the tube immerged in quicksilver. By this arrangement, the sudden and violent expansion, which takes place at the moment of the combustion of the gases, is relieved by the elasticity of the spring, which, by yielding, allows the glass tube to be heaved up a little way, without being driven from its situation. The success of the experiment is thus secured; and all danger of accident to the apparatus is effectually prevented.

Jameson's Elements of Geognosy.

Dr. JAMESON's "*Elements of Geognosy*," being vol. iii. and part ii. of the System of Mineralogy, is a work "entitled to our notice." "This volume," says Dr. JAMESON, "of the System of Mineralogy, is to be viewed as a sketch of the science of Geognosy in its present state. It contains a statement of the best-ascertained facts respecting the aspect of the surface, and the structure of the crust of the earth, and a few inferences which appear to be legitimate, with regard to their mode of formation. It is intended also as the textbook for my lectures on "Geognosy." The following are the subjects of the different chapters: Chap. 1. "Description of the surface of the earth." Chap. 2. "Effects of water on the surface of the earth." Chap. 3. "Internal structure of the earth." Chap. 4. "General account of the different formations, in regard to their succession and stratification, and this illustrated by a short description of the Hartz and the Saxon Erzgebirge." Chap. 5. "Theory of the diminution of the waters of the globe." Description of overlying formations. An investigation of the contents of the original waters of the globe, during the different periods of the earth's formation. "The division of rocks into five classes." Chap. 6. "Primitive rocks." Chap. 7. "Transition rocks." Chap. 8. "Floetz rocks." Chap. 9. "Alluvial rocks." Chap. 10. "Volcanic rocks." Chap. 11. "Mineral Repositories." Chap. 12. "Relative age of metals." These are succeeded by tables, of the relative antiquity and geognostic relations of simple minerals, of the heights of mountains, hills, and lakes, and of volcanoes.

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An Account of two Cases of EXTRA-UTERINE CONCEPTION; *in each of which the Fœtus was extracted by an Operation with success.*

By WILLIAM BAYNHAM, *of Virginia, Member of the Royal College of Surgeons, London. Communicated to* J. A. SMITH, *Professor of Anatomy and Surgery in the University of the State of New-York, and Member of the Royal College of Surgeons, London.*

CASE I.

IN the year —— Mrs. Cocke became pregnant with her third child, and without experiencing any remarkable inconvenience during pregnancy, was seized at the proper period with symptoms of labour. She had her mother with her, who is an old and experienced midwife. The labour pains were regular, but no coloured discharge or waters ever appeared; nor could the child be felt through the os uteri, which was sufficiently dilated, and within the reach of the finger, although its motion was plainly distinguishable by the application of the hand on the belly of the mother. At first the child showed itself to be strong and vigorous, but at length, from the cessation of its former movements, the mother became sensible that it was dead; after which the pains grew gradually weaker, and at the end of the second or third day finally left her, much weakened and exhausted. She remained in a languishing and declining condition, with some, but not much diminution of the tumour in the abdomen, for more than twelve months, when she got better, and gradually recovered a good state of health, which she continued to enjoy uninterruptedly during several years. Two or three physicians attended her during her labour, and at different times subsequent to it, who differed in opinion with regard to her disease; one only believing her to be with child, the others supposing her case to be a dropsy. In the spring of 1786 she consulted me; at which time I received from herself and her mother the relation before stated, together with a minute and circumstantial detail of other particulars; such as her well remembering the time when she quickened, the motion of the child afterwards, the swelling of the breasts, the flow of milk when the labour went off, &c. From all which I did not hesitate to pronounce her case to be that of an extra-uterine conception. She was now perfectly well, rather corpulent, and felt no inconvenience from the tumour, except what was naturally to be expected from its size and weight: I therefore told her she must be content to wait the return of a *second labour;* that at some future day nature, become weary of her burthen, would in some way or other manifest a disposition to rid herself of it, although it was impossible for me to say when or how; that I should then be glad to hear from her; and advised her in the mean time to live as usual, and to think as seldom as possible of her uncommon situation.

In the autumn of 1789 she was seized with the influenza, which was then epidemical in this State; during which, either from the frequency and violence of the cough, or from some other cause, the abdominal tumour became painful, and continued to be a little troublesome to her until the beginning of April, 1790, when she sent for me. The tumour was now more prominent towards the left side than it had been, and was pointed in the middle, where the integuments were inflamed, of a dusky brown colour, and showing a disposition to gangrene; at the same time I thought I could perceive an obscure fluctuation underneath, although from the thickness of the adipose membrane I could not be certain. On the 18th I made an incision into the tumour where it was most pointed, but no matter issued, although I had gone to the depth of two or three inches. Having neither books to consult on the subject, nor medical friends to advise with, my fears overcame my resolution. I desisted, and concluded that it would be most prudent and safe to wait the further efforts of nature, which evidently pointed towards the expulsion of the child, and which I thought might possibly be effected at no distant period through the opening I had made. I dressed the wound with digestive, directed a plentiful use of the bark (of which she took a very great quantity), and recommended a suitable regimen. By the end of summer the wound was nearly closed, but her health was declining; she afterwards became hectic, her appetite bad, and what little nourishment she did take, the stomach was unable to retain, being almost immediately thrown up again, with high fever, night sweats, and troublesome cough. In this situation, believing her dissolution inevitable, unless the cause could be removed, my fears now gave way to a wish to preserve the life of an amiable woman; and although unsupported by authorities of any kind, and unaided by the presence of any practitioner whose counsel could assist or direct me in the operation, I determined at all hazards on the extraction of the child: accordingly, on the 14th of January, 1791, I made a deep incision through the former opening, sufficient to admit the finger into the cavity of the tumour, and with that as a director, cautiously extended the incision upwards and downwards on the left side, from nearly opposite to the umbilicus, obliquely across the rectus muscle towards its inner edge, and thence almost to the pubes, and in the direction of the linea alba, taking particular care in extending the incision upwards and downwards, not to open the cavity of the peritoneum. Through this opening I happily succeeded in extracting the fœtus, which, however, could not be done but in parts, in consequence of putrefaction having taken place some time prior to the operation.. This necessarily took up much time, rendering the operation extremely tedious and fatiguing to me, and painful to the patient. Her happy recovery, and the enjoyment of good health ever since, have, however, amply compensated her for the pain of the operation, which she sustained with an uncommon degree of patience and fortitude, and me for my care and attendance, having visited her at the distance of thirty miles every other day for many weeks during the cure. From the different portions of the child, I judged it to have been of the usual size of a full-grown fœtus; and that the putrefaction had not commenced until, by the first incision, air had been admitted into the cavity in which it had been so long quietly lodged. The heart, with a portion of lungs attached to it, was whole and entire, and still retained its natural shape and colour. Some of the bones were denuded of their periosteum; but those which form the basis of the cranium, all the vertebræ, and a few others, were strongly connected by their respective ligaments. The cavity in which the fœtus had lain, apppeared to be, and I have no doubt was separate and distinct from that of the peritoneum. The cyst which formed this artificial uterus was of considerable thickness, very firm, and its internal surface lined with a thin incrustation of osseous or calcareous matter, which was separated as the cavity gradually contracted, and was thrown out in small portions at every dressing; together with some of the small bones of the hands and feet, which were occasionally extracted with the forceps, until the wound was nearly healed. After the operation I poured into the cavity a sufficient quantity of the samaritan balsam, luke warm, and well charged with the powder of Peruvian bark; covered the wound and parts adjacent that were in a state of inflammation with a cataplasm of bread and milk, in which was mixed a tea spoonful or two of red bark in powder, and secured it by a broad band carried once round the body, and pinned in front. I directed wine and bark internally, with an opiate at night, and a light nutritious diet, and ordered injections to be used occasionally, to procure regular alvine discharges. She never vomited after the operation; she rested well, and suffered very little pain from the wound; her fever and cough gradually abated, her appetite returned, and in the course of a few months she was completely recovered.

At the time she became pregnant she had two daughters living; she was then in her year, and it was upwards of ten years afterwards when I extracted the child. She is now alive and in good health, but has never bred since.

CASE II.

On the 24th of January, 1799, I was sent for by Mrs. Washington, of Hayfield, in the county of Fairfax, Virginia, to a servant of her's, a young woman who had been with child, as she asserted, eighteen months; she had heard that I delivered a woman who had been with child for ten years, and was willing to submit to any operation that I might think necessary for her relief.

She was taken in labour last March, as she thought; the midwife was sent for; but after being a few hours in great pain, the pains left her; she was at the same time taken with the menses, which continued with her for nine weeks, when they left her, and she has had them regularly every month until the last period. She grew less every day, yet kept tolerably well, though oftentimes in a great deal of pain. About six weeks ago she was taken worse than usual, and has been wearing away ever since, having no stomach to eat any thing, always complaining of so heavy a load at the bottom of her belly, that when she stands up she is obliged to hold it up with her hands. I found her labouring under a hectic fever; she was much emaciated, and confined to her bed.

The abdominal tumour was equal and uniform, reaching half way from the umbilicus to the scrobiculus cordis, but more prominent in the umbilical region, where the head of the fœtus was plainly to be distinguished by the hardness of the feel.

I performed the operation on the 6th of February, 1799. I carefully made an incision through the integuments and muscles in the most prominent part of the tumour, thereby exposing the head of the child, and cautiously extended it upwards and downwards by the direction of the finger introduced into the cavity, so as to reach from a little above the umbilicus nearly to the pubes, on the left side, close to and in the direction of the linea alba; by which a sufficient opening was made to extract the child with ease. It was of the common size, and sound, except on the head, where a small portion of the scalp was in an incipient state of putrefaction. A large branch of the internal mammary artery was divided; but it was readily secured by the needle and ligature. No liquor amnii or other fluid was found in the cavity, which closely embraced the fœtus, and to which it was attached and glued, as it were, by a sort of cement of a greyish cast, of the nature of that kind of mucus which is found on the surface of the bodies of new-born infants; so that I was obliged to insinuate my hand, and with my fingers carefully separate the head, body, and arms from their attachments. The funis was small and short, connecting the child with the mother by three or four ramifications, inserted low down in the anterior part of the cavity in which it was lodged. The adhesion was so strong that I did not think it adviseable to use any force to separate it; choosing rather to leave it to nature to dislodge it. Some pieces were thrown off at almost every dressing; but it was upwards of a fortnight before the string came away; some days after which I left the patient to the management of the family, as the wound was by that time healing, the cavity filling up and closing fast. The same dressings were used in this case as in the first. An anodyne was given immediately after the operation, repeated again that evening, and for three or four nights afterwards; it was then discontinued, and the Peruvian bark given three or four times a day. The bowels were carefully kept open from the second day after the operation, first by emollient injections, afterwards by the occasional use of an infusion of senna, manna, and salts. The diet was at first light and slender, but afterwards changed for one more generous and liberal. She soon got perfectly well, and is now alive and in good health. I believe she has not been again pregnant.

I am inclined to think cases of extra-uterine conception are not so rare as is generally supposed, although I find no account of any operation for the extraction of the fœtus in all my reading; nor did I ever meet with a case where the fœtus had grown to full size and maturity, until that of Mrs. Cocke presented itself. They are, I am apt to believe, often mistaken for other diseases; and, being treated accordingly, the patients fall victims to the ignorance, obstinacy, or incredulity of the practitioners attending them, as has already been mentioned in the case of Mrs. Cocke. In the case of Mrs. Washington's servant, the physician, an old experienced practitioner of great respectability in his profession, who attended her for several months, declared it to be a case of schirrous uterus, and gave it up as incurable. Here the poor woman's life was saved by her having heard of my first operation on Mrs. Cocke; and firmly believing herself to be in the same situation, declared she was confident I could relieve her if sent for. Fortunately for the woman, her mistress had humanity and liberality enough to listen to her repeated declarations of this kind, and sent for me to attend her at the distance of nearly an hundred miles.

I was called to another woman in the Northern Neck some years ago, who was then afflicted with an ascites, and much emaciated with hectic fever; but who also declared that she was pregnant, and had overgone her time several months. I tapt her, and drew off a large quantity of thick purulent fluid. After emptying the cavity, I could plainly distinguish within it an irregular moveable tumour low down on the right side.

This was very probably a fœtus of an extra-uterine conception burst forth from the sac or cyst in which it was originally contained, lying loose among the abdominal viscera, and perhaps in a state of decay and putrefaction; which latter circumstance would, in all probability, commence from the date of the child's emancipation from its artificial uterus, and might be the cause of the woman's subsequent pain and ill health, and the collection of purulent fluid in the abdomen.—It is very possible, indeed I think it more than probable, that another lady for whom I was consulted laboured under a case of this latter kind also; and the reason why I did not propose nor consent to her being tapt when mentioned in consultation, was the unsuccessful result of this operation in the last case; from which the woman found no relief, but gradually declined, and died in a few weeks after the operation.—In the course of last summer I was applied to by a woman whose situation, I am almost positive, will eventuate in another proof of such cases being more frequent than has been generally supposed, and of the propriety of medical practitioners directing their attention to this subject.

In performing the operation for the extraction of an extra-uterine fœtus, the first and great object should be, to avoid making a communication between the cavity of the abdomen, and that in which the fœtus is lodged, (which last is separate and distinct from the first) by extending the incision beyond the adhesion of the sac to the peritoneum; thereby making unnecessarily a penetrating wound into the cavity of the abdomen, and subjecting the patient to all the miseries of a large purulent discharge among the abdominal viscera, instead of the matter passing out as it ought to do, (and as it will do if this caution is rightly observed) through the opening made into the cavity, proper to the child only, and which opening should be no larger than will be sufficient for the easy extraction of the fœtus. It appears to me, that the success of the operation depends entirely on this; for the particular place where the incision is to be made is not so important, and will be pointed out by the tumour itself, viz. in the most prominent part, beginning from above, and carrying it near to, and in the direction of the linea alba; the dressings and mode of treatment after the operation being very simple.

At Dr. Hunter's Lectures in London, I had seen one instance where the impregnated ovum, missing the fallopian tube, had fallen into the abdomen, and had attached itself, and grown among the viscera. The fœtus however was very small, not larger, if I remember right, than one of between two and three months old. I recollect to have seen also the bones of another small fœtus, which my friend Mr. Robert Kent, of London, had shown me, and which his patient had passed per anum, who afterwards recovered. In these cases the fœtus soon perishes for want of nourishment, (like a seed dropt by accident on a barren soil, which just vegetates and dies); and then, acting as an extraneous body, it is afterwards expelled by the operation of nature, peacemeal, through the natural, or some artificial outlets; otherwise the patient dies. In those cases where the fœtus arrives to full maturity, I am apt to suppose that the embryo is stopt in its passage from the ovarium in the fallopian tube, or some other part favourable to its growth, to which it attaches itself; a placenta is formed, and the fœtus is nourished (as in the natural uterus) by an artificial one formed by the ovum, which is gradually enlarged by the growth of the fœtus; and as it enlarges, it probably increases in thickness, adhesions take place with the parieties of the abdomen and the neighbouring parts; thus enclosing the fœtus in a place of perfect security, where it is supplied with nourishment from an entirely new set of vessels. The woman, with more or less inconveniency, goes on to her full time, and then is taken with labour pains, unaccompanied with other usual symptoms of parturition; as in the cases already related. Such gradual enlargements, and similar attachments are found to take place in dropsies of the ovaria.

Note. We have been informed by Mr. Smith, that he has lately examined the woman on whom he operated, whose case is mentioned at p. 54 of this volume; that he found a tumour about the size of a goose's egg, which communicated externally by a small fistulous orifice, from which an inconsiderable quantity of matter occasionally oozed. The woman is fat and hearty, and experiences no inconvenience from it. *Editors.*

To the Editors of the Medical and Philosophical Journal and Review.

GENTLEMEN,

I send you the enclosed for publication, without any comments of my own. I confess I am wholly unable to account for so extraordinary a production of nature. Should you not have previously received it, I trust it will not be unacceptable; and hope some able physiologist will attempt to elucidate the subject.

Yours, &c.

J. PERCIVAL.

Extract of a Letter from Dr. GAITHER, *of Washington County, Kentucky, to the Editor of the Western World, dated Springfield, April* 26, 1809.

Intra-Muscular abdominal Monsters.

BELIEVING it the duty of professional men to lay before the public such occurrences in their practice, as, from their singularity, may either awaken useful inquiry, or substantially promote the happiness of man, by increasing the catalogue of facts whence a knowledge of the animal economy is derived; I, who am a young and diffident practitioner in the science of medicine, beg leave to submit to the public a *fact* of that description. It is so contrary to the common course of nature, and to the prevailing and generally received opinions of the learned in the science of physiology, as to justify the conjecture that *nature* has yet many mysteries in reserve of great importance to the human family.

When, or in what manner she may please to disclose them; whether she will surrender them voluntarily and in connection, or whether they are to be unfolded singly, and by seeming accident, and at distant and irregular intervals, remains equally a mystery. Possibly the learned may avail themselves of the instance I am about to relate, either to elicit or extort from her many valuable secrets. With that view and hope it is submitted to them. It will be faithfully and accurately related, and can be in all its material parts substantiated by many persons of unquestionable veracity.

On the 7th of April, in the county of Washington, I was called to visit a female child, the daughter of John Milbourn, jun. The child was two years and nine months old, and was supposed to be affected with the ascites. She died about three hours after my arrival.

Her parents gave me a detailed account of her case, and its various symptoms. I was by no means satisfied that it was a real dropsy, though there was great tumefaction and tenseness of the abdomen, and fluctuations were evidently felt in it when pressed by the hand. But as some of the symptoms could not be referred to that complaint, and others were equivocal, I could not but suspect that her disease had been mistaken. I prevailed on her parents to permit an examination. The operation was performed in the usual way, by a longitudinal incision, passing from below the sternum and reaching nearly to the pubes, and a transverse one passing from the umbilicus to the anterior and superior spinous process of each ilium. A cavity was opened, about half the distance between the abdominal cavity and the exterior surface, that discharged between three quarts and a gallon of yellow water, which smelled like rotten eggs. Within the cavity was found a monster, or imperfect child, and also an animal substance of a whitish colour. The monster weighed one pound and fourteen ounces—the substance weighed two ounces, was rather of an oval figure, and was connected to the child, from which it was taken, by a cord that had some faint resemblance to the umbilical. On one extremity of the substance is a small teat or protuberance, about half an inch long, and between one-fourth and one-half of an inch in diameter; and immediately by it is hair of a darkish or auburn colour, about an inch and one-fourth long. The substance is covered by an epidermis.

The monster occupied part of the epigastric and the umbilical regions. It was not connected to the inner surface of its cavity by a cord, or any visible medium. Whether a cord or other medium of connection had existed and been destroyed by putrefaction, (which, from the smell of the fluid and other appearances, had commenced) could not be positively ascertained. That there must have been some medium of connection I am confirmed, as well by the universal course of nature and analogy on this subject, as by an appearance at the articulation of the cervical and dorsal vertebræ, resembling faintly the divided funis.

The position of the monster in its envelope was awkward; its thighs drawn up to its abdomen, and attached to it in places; the left resting on the shoulder, and reaching as far as the back part of the head; the right resting or pressing on the back of the right hand. The bones of each thigh have perforated the flesh at the knee, and are about half an inch out. The left leg is imperfect, lies back along the thigh, to which it has grown. The right leg is also imperfect, its foot is suspended over the head. On one foot are three toes; on the other a small appearance of two. From the knees to the shoulders there is considerable perfection of form. Its sex is indistinctly marked; the indications are feminine. The left arm should rather be called a stump than an arm; it has no hand—at the end of the stump is a nail. The right arm is large and long; it has three fingers and the thumb. The head is very imperfect; it rests upon the breast between the knees. It has neither ears nor eyes, or appearance of any substitute for either—no mouth, nor any thing that has a near resemblance to it. There is on the left side of the face, or rather that region of the head which the face should occupy, a small prominence, which contains three teeth, a canine and two incisores; they are about the size of the teeth of a child of two years old. This prominence or mouth, if it may be so called, has no aperture. On the back part of the head was hair of a dark, or rather of an auburn colour, eight or nine inches long. The body of the monster was seven inches long, and ten inches in circumference: the thighs six in length, and eight in circumference: the arm is five; the stump not quite four inches in length.

The interior of the cavity which contained the monsters resembled the membrana decidua. There was no vestige of membrane peculiar to the monster discovered. Having explored this cavity, and dislodged its contents, I extended the incision through the muscular partition into the abdominal cavity, and examined the viscera. They were rather pale, otherwise natural.

The little girl that those monsters were taken from for about nine months was healthy. Her parents discovered, when she was only a month or two old, something hard within the abdomen, which continued to increase. After this time she became less healthy; but her complaints were those incident to all children. About nine months prior to her death she began to decline, and became emaciated; her appetite continued strong; her longings and desire for ardent spirits were great; she would become intoxicated if indulged in the free use of them; it took a considerable quantity to affect her; she drank freely an hour before her death. I believe it was the use of spirits in part that supported her so long. She was of the ordinary size of children at her age, had dark hair and eyes, and would have been handsome but for a gloom and melancholy that sat upon her countenance, which made her appearance peculiarly interesting—She looked like the child of grief. Her countenance exhibited evidences of a good understanding, and her little tongue confirmed it.

EDWARD B. GAITHER.

I certify, that I examined with anxiety and attention the monster above described, and also the substance, and believe the description to be accurate. I also conversed with the young gentleman who was present and assisted at the operation—his statements were correspondent with the above narration of facts and circumstances. In the veracity of the Doctor and young gentleman with whom I conversed I have most absolute confidence.

JNO. ROWAN.

April 26, 1809.

I, THOS. J. COCKE, do certify that I have examined the above described monster, and that it answers to the description given; and that I have the fullest belief of the whole of the facts as related.

April 28, 1809.

I do certify, that I have particularly examined the monster above described, and it corresponds with the above statement; and I have the fullest confidence that all the circumstances as stated are correct.

JOHN CALHOON.

April 28, 1809.

Note. Tumours containing hair, teeth, bones, &c. have been frequently discovered in the examination of female subjects after death. Mr. Coleman, the veterinary professor in London, showed that such tumours were not peculiar to the female sex or human race, he having met with one of that nature in dissecting an horse. After that, a case occurred in France, in which something very like a fœtus was discovered in the abdomen of a lad. Any attempt to explain these phenomena in the present state of our physiological knowledge must, we fear, be abortive. We know, indeed, that arteries, under certain circumstances, have the power of secreting bone, where it does not naturally exist; but we can hardly conceive that the monster described by Dr. Gaither was the production of mere morbid action in the blood vessels of the part. *Editors.*

A CASE *of* MORTIFICATION *arrested by the application of Blisters.* *By* J. AUGUSTINE SMITH, *Professor of Anatomy and Surgery in the University of the State of New-York, and Member of the Royal College of Surgeons, London.*

ON Thursday, the 23d of July, 1807, I was requested to visit a Mrs. Gressit, living in Gloucester county, Virginia. It not being in my power to go that day, I did not see her until the next. When I arrived, I found the lower part of her right leg and ankle very much swelled, particularly the latter, which had likewise numerous vesications filled with a bloody serous effusion : these did not extend beyond the part where the tendo achilles commences. Her pulse was small, and about ninety. The account I received was as follows; that returning on the preceding Wednesday from church, in stepping from a piece of timber about eighteen inches high, her foot turned, and she fell. That her foot was so twisted by the accident from its natural position, that her companions thought it was dislocated, and proceeded to reduce it, in which they supposed they succeeded. That she was then carried to the nearest house, and that brown paper and vinegar were immediately applied. That on the next day she took a dose of salts, which operated freely.—Her health, she said, had been indifferent for two years past, at which time her menses had disappeared; and that since she had been occasionally affected with sores, which were healed with difficulty.

Under these discouraging circumstances I gave a very unfavourable prognostic, being apprehensive my patient could with difficulty escape, even with the loss of her leg. I directed her to take as much of the cinchonæ flavæ as her stomach could bear, to drink freely of brandy and water, (as wine could not be procured) and to eat as much stimulant diet as she could. As she suffered no pain I did not administer opium. Her leg I directed to be dressed with the ung. resinæ flavæ.

25th. She had slept well, and the bark agreed with her stomach, and her pulse was rather fuller; but the tumefaction of her leg had increased, and the vesications had extended higher. Her leg was now dressed with equal parts ung. resinæ flavæ & sp. terebinth. The other remedies were continued.

Returning home, and reflecting upon the case, I recollected that while in London I had received a letter from my friend Dr. Dorsey, now adjunct Professor of Surgery in the University of Pennsylvania, informing me that Dr. Physick had used blisters in such cases with great success. I determined, if I found her no better the next day, to try them.

26th. Worse, the vesications being darker coloured, and more extensive. A blister about two inches wide, and sufficiently long to encircle the whole leg, was applied immediately above the vesications. No alteration in the other remedies.

On the 27th I was much pleased to find the tumefaction of the leg considerably diminished, and that the mortified spots, for the first time, showed a disposition to separate. The sp. terebinth was now omitted, other remedies continued.

The next day a blister was applied in the same manner around her foot. The dressing to the leg was continued for a few days, and then, any farther attendance being deemed unnecessary, I took my leave. I have since been informed that she completely recovered the use of her foot.

Note. The above is one of several cases which have come to our knowledge of the efficacy of blisters in arresting the progress of mortification. We consider them far superior to any other application, and particularly valuable, because they may be employed in conjunction with the other remedies usually recommended and used in such cases. *Editors.*

For some time past the public has been amused by various ludicrous references to Gall's Craniology, as it has been termed. We have it now in our power to gratify our readers with a most learned investigation of all Dr. G.'s opinions with regard to the Brain.

From the Edinburgh Medical and Surgical Journal.

Report on a Memoir of Drs. GALL *and* SPURZHEIM, *relative to the Anatomy of the Brain. By* M. M. TENON, PORTAL, SABATIER, PINEL, *and* CUVIER, *presented to, and adopted by the Class of Mathematical and Physical Sciences of the National Institute.*

THE physical class appointed M. M. Tenon, Portal, Sabatier, Pinel, and Cuvier, to report upon a memoir, entitled, Inquiries into the nervous system in general, and the brain in particular, by Drs. Gall and Spurzheim.

Your committee must confess, that they had at first some doubts about undertaking its examination.

The class has always adhered to a very wise rule of giving no opinion upon works already submitted in print to the great tribunal of the public; and it might be supposed, that the anatomical doctrine of Dr. Gall has obtained, by means of the lectures which he has delivered in the principal cities of Europe, and by means of the numerous extracts from them, published by his pupils, a degree of publicity almost equivalent to that of authentic publication.

Besides, this anatomical exposition of the nervous system is generally supposed to be intimately connected with, and, in fact, is connected to a certain degree, by its author, with a physiological doctrine which he teaches, concerning the special functions of the different parts of the cerebral organ, a doctrine which in no way comes under the cognizance of the class, since it ultiin preserving the traces of these impressions, and in reproducing them with greater or less promptitude, distinctness, and abundance, when the mind requires them for its operations, and when the laws of the association of ideas recall them; lastly, in transmitting to the muscles, always by means of the nerves, the desires of the will.

Now these three functions suppose the mutual, but always incomprehensible influence of the divisible matter and the indivisible mind (moi); a hiatus in the system of our ideas never to be supplied, an eternal stumbling-block of all our philosophies. But there is another difficulty not necessarily connected with the former. We not only do not comprehend, and never shall be able to comprehend how any traces impressed on our brain can be perceived by the mind, and produce images in it; but however delicate our researches, these traces are in no way visible to our eyes, and their nature is perfectly unknown to us; although the effect of age and of disease upon the memory does not permit us to doubt of their existence, or of their seat.

It seemed, at least, that the action of the nervous system upon the organic life, would be more easily explained, as it is entirely physical, and we might expect, by means of investigation, to discover in this system some texture, some intermixture, or direction of parts which would render it more or less analogous to the vascular or secreting organs. There was especially no reason to doubt, that it would be possible to unfold their different portions, to assign their connections, their relations, and respective terminations, as easily as in the other systems.

This, however, has not happened. The texture of the brain, of the spinal marrow, and of the nerves, is so fine, so soft, that all that has been hitherto said of them is blended with conjectures and hypotheses; and the different masses which compose the brain are so thick, and have so little consistence, that the greatest dexterity is required to show all the parts of their structure.

In short, none of those who have examined the brain, have succeeded in establishing a rational and positive relation between the structure of that organ and its functions, even those which are most evidently physical: the discoveries hitherto made known with regard to its anatomy, are confined to some circumstances regarding the form, connections, or texture of its parts which had escaped the observation of preceding anatomists; and whenever any one has supposed that he had proceeded farther, he has only introduced, between the well known structure and its common effects, some hypothesis, scarcely capable of satisfying for a moment even the least sceptical minds.

All the actual discoveries which have hitherto been made, may be reduced to new methods of dissecting the brain, new connections and directions perceived between its different masses, and the organic elements composing them, or new peculiarities observed in some of its parts.

We are far, however, from despising these results; they direct us to the only route which may at last conduct us farther; and although we do not yet know the whole length of this route, we are satisfied, that every step made in it brings us nearer to its termination, by some fraction of its length.

We proceed, then, to explain and examine, under these three heads, of method, connection, and peculiarities, the discoveries announced by Drs. Gall and Spurzheim.

Anatomists know that there are three principal methods of demonstrating the brain.

The one most generally practised in the schools, and described in books, is that of Vesalius, which consists in removing successive layers of this organ, and in pointing out what presents at every cut. It is the one the most easily performed for the sake of demonstration, but the most difficult to be followed in the mind. The true relation of those parts, which we see always cut, escapes, not only the pupil, but the teacher also; it is almost as if one were to divide the trunk of the body by successive sections to point out the position and form of the lungs, heart, stomach, &c. This method, however, is still almost the only one which is employed in the work of Vicq-d'Azyr, the most splendid, and one of the most valuable which has appeared on the subject of the brain.

The second mode, which deranges much less the organ, the structure of which it is intended to explain, is that of Willis, which, as far as we can judge by the obscure description of Galen, very much resembles that employed by the ancients. After removing the *pia mater*, the posterior lobes of the brain are raised, we penetrate between the *tubercula quadragemina* and the *fornix*, and cut its anterior pillar; after detaching the lateral parts of the hemispheres, they are turned forwards: in this manner we have a good view of the under part of the *fornix* and *corpus callosum;* we preserve entire, the great and small tubercles of the interior; but the thickness of the hemispheres renders it more difficult to practise this mode in man than in other animals.

The third method was very long ago sketched by Varolius, and afterwards more fully detailed by Vieussens. By this mode the under part of the brain is first examined, and the *medulla oblongata* is followed across the *pons varolii*, the *thalami optici*, and *corpora striata;* its fibres are seen expanding to form the hemispheres; it is even possible to stretch out the hemispheres by removing their lateral attachment to the crura cerebri; to divide longitudinally the spinal marrow and the cerebellum; and then each half of the former is seen forming a pedicle, which is implanted into the side of the hemisphere, as the stem of a mushroom is fixed into its *pileus.*

This mode possesses the very great advantage of allowing us to follow more easily the direction of the medullary fibres, the only circumstance which can afford us any idea with regard to the course of the cerebral functions; and it is probable, that it would have been more generally adopted, if Varolius had not represented it in a very coarse figure, and if the work of Vieussens had not always remained, for what reason it is difficult to determine, in a kind of discredit which it did not deserve.

It is nearly this method of Varolius which is followed by Drs. Gall and Spurzheim, and which a part of their memoir is intended to defend; a labour, indeed, very useless, for so complicated an organ as the brain ought to be examined on every side; it must be cut into in every way; and whenever any plan is discovered which points out some new circumstance, it furnishes an important addition to anatomy.

We shall therefore judge of their mode by the results; and, for that purpose, we shall detail and compare them with those which have been obtained by preceding anatomists.

It is well known, that the opinion most generally received with regard to the minute organization of the brain, is, that the cortical substance of the hemispheres, and of the cerebellum, being almost entirely vascular, is a sort of secreting organ: that the medullary substance, almost every where of a fibrous appearance, is a collection of excreting vessels, or at least of conducting filaments; that all the nerves are emanations from this substance, bundles of these vessels; that the *medulla oblongata*, and spinal marrow, is itself a bundle larger than the others, from which the different pairs of spinal nerves go off in succession: lastly, that the nerves called cerebral, are the first which go off from the great medullary mass of the encephalon; consequently, it is believed that all the influence of the nervous system upon organic life, as well as all the efforts of the will, descend from the brain along the nerves; and that the impressions received from the external senses, re-ascend by the same route; but by a singular contradiction, at the same time that they make the medullary substance, and consequently the nerves, originally extend to the whole of the cortical substance, several physiologists think themselves obliged to search for some circumscribed place from which all the nerves emanate, or, what signifies the same thing, in which all the nerves terminate; in short, for what anatomists call the *seat of the soul.*

It must be allowed, that this has been for a very long time the opinion most generally received, and that it is still very prevalent; although the most prudent philosophers have never brought it forward but as an hypothesis very slightly supported by facts.

Several of its partizans, however, have allowed themselves to be led into doubts and contradictions. Haller, for example, says in one place, that he cannot suppose that medullary fibres originate any where else than in the brain;* in another, that every nerve comes ultimately from the medulla of the brain or cerebellum;† while, in a third place,‡ he supposes that the cineritious matter of the spinal marrow may form them in the same manner as that of the brain.

In fact, this distribution of the cineritious matter in different parts of the nervous system, was one strong argument against the exclusive importance ascribed to the encephalon, and to it might be added several others.

It might be remarked at all times, that the action of the nerves upon organic life continues for some time after the brain no longer contributes to it. Well known experiments upon insects and worms proved, that if in man, and the other animals where the brain is very large, this organ is necessary to the functions of animal life, it is not always so in those species where it is of less size, and that in some of these we may even instantaneously produce, by dividing it, two centres of volition and sensation.

It has also been known for a very long period, that the spinal marrow does not diminish in proportion to the nerves which go off from it, as it ought to do if it were only a bundle of these nerves sent from the brain; that, on the contrary, it swells at certain places, from whence the largest nerves go off. Prof. Soemmerring has recently remarked, that the size of the *medulla oblongata* in animals is not in proportion to that of the brain as it ought to be, if it were a congeries of excretory tubes from the brain, but that, on the contrary, it is often in an inverse proportion; the successive investigations of Monro, Prochaska,

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* Phys. iv. p. 385.

† Ibid. p. 393.

‡ Ibid. p. 384.

Reil, have given us ideas of the structure of the nerves quite different from those which we would be obliged to form if we were to derive all of them from the medullary substance of the encephalon, and by it from the cortical substance. Several physiologists, therefore, within these few years, have again begun to consider the nervous system as a net-work, all the portions of which contribute, in a certain degree, and especially according to their size, to the organization and functions of the whole; and not as a tree which, having only one trunk, distributes itself into branches and twigs, after the manner, for example, of the arterial system.

Drs. Gall and Spurzheim, in adopting this opinion, do not adduce new proofs of it, but only repeat those which we have stated, and which had been brought forward many years before.

It appears, that in Germany and other places, various objections have been made to their system which we would not have made, but which they have taken the trouble to answer.

When they, for example, described, that in the fœtus without a head, the nervous system performs its functions of organic life without the assistance of the brain, it was opposed to them, that the acephali are only fœtuses where the brain has been destroyed by the effects of dropsy. This objection, which is true in some acephali, certainly does not apply to all; and it is by no means rare to see some which have been completely developed, although they do not show the smallest mark of having ever had either a head or any of the upper parts of the trunk.

We, therefore, readily agree with Drs. Gall and Spurzheim, with regard to the general idea which they entertain, as well as a great many anatomists, of the nervous system. But while they, with so many others, consider it as a net-work, they have some particular ideas concerning the links and knots of which this net-work is composed; and here the part of their doctrine begins which is peculiar to them.

As far as we have been able to understand, it appears to us, that it may be reduced to the ten following heads, or propositions :

1. The cineritious matter is the *matrix of the medullary filaments ;* wherever it exists, these filaments are produced ; it exists wherever they are produced. Whenever a medullary bundle crosses cineritious matter, it is enlarged by filaments received from the latter, and none of these bundles enlarge without the concurrence of this matter, whether it form a sensible enlargement, or only follow and accompany the medullary bundle.

2. The spinal marrow is not a bundle of nerves descending from the brain. The spinal nerves arise by filaments, some of which ascend, others descend ; this is observed especially in brute animals. The cineritious matter of the internal part of the spinal marrow is the matrix of these filaments ; the spinal marrow swells wherever it gives off a pair of nerves, and the more the larger that these nerves are. Thus the spinal marrow of the larger animals, as well as that of insects and worms with red blood, is only a series of ganglia, which give off nerves ; but all these ganglia communicate with each other.

3. The nerves commonly called cerebral, and which arise from the under part of the encephalon, and chiefly from the medulla oblongata, do not come from the brain any more than the others ; on the contrary, when we follow separately the roots of each of these into the substance of the medulla oblongata, we see that they re-ascend from the medulla towards the point where they appear externally, and that they do not descend from the brain, to pass through the medulla oblongata.

4. The brain and cerebellum are themselves only developements of bundles which have come from the medulla oblongata, in the same manner as the nerves proceed from it.

The brain, in particular, arises principally from the bundles called pyramidal eminences, which cross each other in passing out of the medulla oblongata, each going to the opposite side to that from which it comes; they swell out first in crossing the *pons varolii*, again in passing over the tubercles called *thalami optici*, and a third time in those tubercles called *corpora striata*, always from the addition of medullary filaments, which the cineritious matter, contained in these three parts, adds to those they originally consisted of. The cerebellum comes from bundles called *processus cerebelli ad medullam*, or *corpora restiformia*, which receive an addition of filaments once only from the cineritious matter of what is called *corpus ciliare.*

5. These two pairs of bundles, after receiving these additions, after having assumed consequently a diverging direction, terminate each in two lerge expansions, covered externally by cineritious matter, which only here deserves the name of *cortical* substance; and these expansions, folded in various ways, form what are called the hemispheres of the brain, the lobes, and the *vermiform processes* of the cerebellum.

6. From the whole extent of these expansions, other medullary filaments are produced, which converge from the two sides of the brain and cerebellum, towards the middle line, where the filaments of the one side unite with those of the other, and form what are called the *commissures.*

The corpus callosum, the fornix and its appendages, form the largest of the commissures of the brain; the anterior commissure is that which joins the middle lobes. The commissure of the cerebellum is composed of the transverse layers of the pons varolii.

7. When we remove or lacerate the converging fibres which go to the corpus callosum, and which serve as a ceiling to the lateral ventricles, there only remains, under the cineritious substance, a medullary part lining it, and following all its folds; and far from forming a solid mass, as has been hitherto supposed, there is always in the middle of each convolution of the brain and cerebellum a solution of continuity, and by employing proper care, we may unfold this portion of the medulla, as we would unfold the cineritious substance if it were alone. In

a word; each convolution is a kind of small purse or canal, closed externally by a double layer of cineritious and medullary matter, and on the side of the ventricle by the converging medullary fibres.

8. As the pairs of bundles forming the brain and cerebellum have their commissures, those which form the nerves have often theirs also, which may be very easily demonstrated in the second, the fourth, fifth, and seventh pairs, and very probably in the others.

9. The ganglia spread over the whole body are small masses of cineritious matter, which certain nerves pass through, and where they are strengthened as the pedunculi of the brain are increased in the thalami optici and corpora striata.

These two pairs of tubercles then are true ganglia to these pedunculi. The cineritious matter of the cortical part of the brain and cerebellum may likewise be regarded as a ganglion of the commissures, or converging fibres. That of the inner part of the spinal marrow forms, in like manner, the primary ganglia of the spinal nerves. The cerebral nerves have probably each of them a particular ganglion, and it is easy to demonstrate it in several of them. The cineritious matter, and consequently the ganglia, may therefore be compared to the mucous expansion which covers all the extremities of the nerves of the skin, of the intestines, and even the pulp of the labyrinth, and the sort of mucous varnish which covers the retina.

10. From these nine articles, all merely anatomical, all more or less susceptible of being verified, a tenth results, which completes, and is the essential character of the anatomical doctrine of Drs. Gall and Spurzheim, viz. that each pair of nerves forms a particular system; that all these systems communicate together, and re-unite in the great cord of the medulla oblongata and spinal marrow; and, lastly, that the brain and cerebellum, far from being the origin or source of this cord, are, on the contrary, an appendix, or sort of *diverticulum*, reserved for certain functions, but receiving an influence from all the

parts of the cord, and exercising one upon them by means of their communications.

We do not think that any anatomist can perceive any obscurity in this new explanation of the ten principal articles premised by the authors of the memoir which we are examining; they have, besides, acknowledged them to be the true expression of their opinion.

It only remains for us, therefore, to say how far they appear to us to be true and new; this we shall do separately with respect to each of them, and with the more interest, as there will result from our examination a kind of treatise, in which the structure of the brain will be considered under different views, more or less important, and abounding with extensive consequences.

But, before proceeding to this, justice requires us to repeat the declaration of Drs. Gall and Spurzheim, that they do not pretend to have discovered many new facts; but that the chief merit which they claim consists in the connection which they have been the first to establish between the known facts, and in the general conclusions which they have deduced from them.

The first article which ascribes as a function to the cineritious substance the giving rise to the medullary filaments, or, as the authors of the memoir express it, the being the *matrix of the nerves*, is, indeed, only another expression for the opinion generally received. Disputes have existed with regard to the texture of that substance. Malpighi supposed it to be formed of small follicles; Ruisch, perhaps with more justice, only admitted a vascular net-work; others are of opinion, that there is also, besides the vessels, a particular substance; but it has been almost always agreed to regard it as a secreting organ, and the fibres of the medullary substance as the excreting organs of the substance which it secretes; it was therefore necessary that these fibres should arise from the cineritious matter. The physiologists who do not believe the nerves to be hollow, but suppose that they have the power of conveying a fluid in the manner that the metals conduct electricity, do not all deny, on that account, that the nerves receive their fluid in the cineritious substance; hence they also think that they arise from it. These, lastly, who ascribe to all the portions of the medullary matter a secreting power, do not pretend to deny, what the eye demonstrates, the close adhesion of the medullary to the cineritious matter of the cortical part of the hemispheres, and the immense quantity of filaments which go out at so many radicles, the cineritious portions of the corpora striata and thalami optici, &c.

Our authors have, therefore, nothing particular in the function which they ascribe to the cineritious substance. Even in generalizing this function to all the parts of this substance, they only express, in more positive terms, what, as we have seen above, Haller suspected to be the case with regard to the cineritious portion of the spinal marrow.

Since this opinion is admitted by so many anatomists, there must be powerful reasons for it; in fact, besides what the eye informs us of the intimate connection of the two substances, the number of arteries which go to the cineritious substance, and which seem almost entirely to compose it, can have scarcely any other object than a copious secretion.

The quantity of cineritious matter dispersed over all the parts of the nervous system, and to which the authors of the memoir have the merit of recalling our attention, would, perhaps, explain sufficiently the functions which the parts of the system exercise without the concurrence of the brain, and would dispense with the necessity of having recourse to a particular power of secretion in the medullary substance, or even in the sheath of the nerve, as Reil supposes.

The *second article* draws a comparison between the spinal marrow of the superior animals, and that of insects and articulated worms, or those with red blood.

It is well known that, in the two latter classes, the brain is scarcely larger than the ganglia, or knots of the spinal marrow from each of which the pairs of nerves go off; that it is from the size of these ganglia, and from their separation, as well as from the smallness of the brain, that we endeavour to explain the divisibility of the individual, which is remarked in all these species, at least for some minutes, and which goes so far in some, such as the *earth-worms* and the *naides*, as to make two lasting individuals from one, by means of division.

Nothing resembling this had been perceived in man, whose spinal marrow has no sensible contraction, and only swells out at those places from which it gives off nerves to the arms and thighs; but Drs. Gall and Spurzheim have showed us a preparation of the spinal marrow of a calf, in which is observed a kind of slight swelling between each pair of nerves. It would be curious to ascertain exactly in what animals this structure is found, and if it has any relation with the power of executing certain voluntary actions, without the brain; if the turtles, for example, which live and walk several months successively without this viscus, have the spinal marrow more knotty than the other animals with red blood, &c.

One of our number has begun inquiries with this intention, which have not afforded him sufficient results to be laid before this class; but he has already ascertained that there are no sensible knots in quadrupeds even very nearly allied to the calf.

The *third article* is subdivided, for examination, into as many heads as there are pairs of nerves.

The general result which the authors propose to prove is, that all the nerves arise from the medulla oblongata or spinal marrow, and not from the brain.

There is no difficulty as to the spinal nerves, which are only conjectured to come from the brain, but the roots of which undoubtedly no human eye can trace into it, nor even perceive a tendency to run into it.

There can be no more with regard to the last pairs of the encephalon, counting from the par vagum, and under it, for they arise by transverse filaments, as the spinal nerves, although they have not two bundles; and no anatomist has observed these filaments reflected towards the brain after having entered the spinal marrow.

There is still less doubt with regard to the nervus accessorius of Willis, which evidently re-ascends.

We have only, therefore, to attend to the first eight pairs, reckoning the facial nerve as a separate pair.

The *seventh* pair of Willis is, in fact, now generally acknowledged as forming two nerves, distinct at their origin as well as in their course. The soft portion, or the *auditory nerve*, arises transversely from the *corpus restiforme*, called also *processus cerebri ad medullam*. This nerve has been for a long time considered to be formed by the small white filaments traced over the ceiling of the fourth ventricle, and this is still the opinion of Haller,* of Vicq-d'Azyr,† and of Soemerring.‡ However, as these filaments vary in number, and even in direction—as a part of them are sometimes seen re-ascending towards the corpus restiforme, or passing through it towards the pons varolii,§ and as it is not very rare to find them wanting, their continuation in the auditory nerve has begun to be doubted. Prochaska,‖ the two Wenzels,¶ &c. have formally denied it. The latter** and Dr. Gall have farther remarked, that these striæ are generally wanting in animals.

The two Wenzels†† observed, in 1791, for the first time, a small grey band slightly prominent, placed also transversely upon the corpus restiforme, and which constantly covers a part of the base of the auditory nerve, which it unites with the fourth ventricle. Prochaska is hitherto the only one who has given a representation of it.‡‡ It is likewise observed in animals; and Dr. Gall, who adopts, in this respect, the opinion of the Wenzels, remarks that it is the more swelled in every species the larger the ears are, and the more acute the hearing is.

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* Phys. t. iv. p. 225.

† Explication des Planches, p. 95.

‡ De Fabrica Corp. Hum. t. iv. p. 256.

§ We have had a very marked example of the latter structure in the course of the researches occasioned by this memoir.

‖ Oper. Min. t. i. p. 388.

¶ Prod. p. 22.

** Prod.

†† Ibid.

‡‡ Oper. Min. t. i. tab. iii. fig. 1.

In the *horse*, the *stag*, the *sheep*, it is a tubercle almost as large as the eminence called *testis*.

We have found this circumstance to be true.

It is besides evident, that if the origin formerly admitted were the true one, the auditory nerve would not, therefore, arise less transversely from the medulla oblongata, and that its visible roots would always come rather from below upwards than from above downwards.

The *facial nerve*, or portio dura of the seventh pair, and the *abductor* or the sixth pair, are then the first which can leave any doubt, whether they arise from the medulla oblongata, or from the brain, from behind or from before.

In man they both arise from the body of the medulla oblongata, immediately behind the posterior edge of the pons varolii, and so near to it, that several anatomists make them derive a part of their filaments from it.

The *facial* nerve, in particular, goes off some lines farther outwards than the other, at the angle formed by the pons varolii and corpus restiforme, at about a line from the point where the *auditory* nerve is detached from the corpus restiforme, which it had, as it were, surrounded.

The *abductor* seems to arise from the fissure which separates the pons varolii from the pyramidal eminences; and there are some anatomists who derive all its roots from the pons, others from the pyramids, others from both parts. There are others again who are not explicit with regard to this point.

According to the idea generally adopted, that the nerves descend from the brain, M. Soemmerring supposes* that the abductor nerve has its roots in the pedunculi, and that they separate from them, turning backwards, after the pedunculi have crossed the pons varolii to form the pyramidal eminences. This is nearly the opinion of Vieussens,* but it is evidently the result of hypothetical reasonings, and not of actual observation.

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* De Bas. Enceph. p. 140.

To ascertain the true direction of the roots of these nerves, it is necessary to have recourse to the herbiferous animals, in which the pons varolii does not cover them, since it is not so large as in man.

This is what Drs. Gall and Spurzheim have done, and they found at once that the *abductor*, in them, arises some way behind the pons varolii, and appears to be the continuation of a small bundle which ascends between the corpus pyramidale and olivare. The filaments which produce it are longer at the back part and shorter before, so that they have, in miniature, the same arrangement as those of the accessorius of Willis. There is no reason, then, to suppose that it descends from the brain.

This observation concludes the discussion whether or not the nerve receives some filaments from the pons varolii; since it is only on account of the breadth of the pons in man that it approaches to its posterior edge.

We have not found any positive traces of this remark in the authors whom we have consulted, but we have convinced ourselves that it is true with respect to the herbiferous animals; and one of our number had even observed it some time ago in the horse. In the carnivorous animals and the ape, the pons varolii and the sixth pair have a greater resemblance to what is observed in man.

With regard to the *facial nerve*, a transverse medullary band is seen in the herbiferous animals, behind the pons varolii, which begins exactly at the outer edge of the abductor, and passes over the root of the *par trigeminum*, or where it is continuous with the auditory nerve. The facial nerve appears to pierce the band obliquely from behind forwards. Thus it would seem to arise from the under part of the medulla oblongata, almost in the same manner as the auditory nerve arises from the upper part, and two pairs of nerves would be formed, the origin of which is, in fact, separated from each other by the whole thickness of the medulla oblongata, although they afterwards approach so as to touch each other.

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* Neurogr. Univ. p. 176.

Nor have we remarked, that any author has pointed out this fact before Dr. Gall; but we are certain of its accuracy, and one of us had seen it, and delineated it some time ago, in the stag, horse, sheep, and rabbit.

The brute animals also show more clearly than in man the origin of the *par trigeminum*, or fifth pair.

It is generally said simply to arise from the lateral parts of the pons varolii, or from the extremity of the pedunculi cerebelli. This is still adhered to by Vicq-d'Azyr* and Meckel.† Haller reckons this pair in the number of nerves, which may come both from the cerebrum and cerebellum.‡ It is, however, ascertained, that it neither comes from the one or the other, and that it may be traced deep into the medulla oblongata, to nearly an inch farther back than where it comes out.

Santorini asserts§ that he has traced the roots of it to above the corpora olivaria, and says, that it is not more astonishing to see this nerve ascend from below, than the *accessorius of Willis;* but he then supposes, that a part of the fibres of the pedunculi which do not enter the corpora pyramidalia, they being, in fact, too small to contain them all, go on further, and then those, among others, are reflected which form this nerve; a very gratuitous supposition, and which nothing sensible to the eye can justify.

M. Soemmerring seems not to have understood Santorini at the time he wrote his treatise *De basi encephali;*‖ but he relates, that¶ accident had made him afterwards trace the origin of this nerve into the substance of the medulla oblongata, even as far as the bottom of the fourth ventricle; and, according to his favourite hypothesis, with regard to the seat of the soul, he conceives that the primary roots of it are bathed in the water of that ventricle.

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* Explication des planches, p. 52. † No. 46 et 47. ‡ Phys. t. iv. p. 387.
§ Observat. Anatom. p. 64, 65. ‖ P. 135. ¶ De fabric. corp. hum. p. 212. No. 7.

Dr. Gall follows, in a steady and sure manner, this deep and low origin of the par trigeminum, as far as between the *corpora olivaria* and *restiformia.* He shows besides, that the breadth and thickness of the pons varolii in man, were the obstacles to its being discovered sooner. In fact, in the herbiferous animals, in which the pons is much narrower, the roots of the trigemini are easily traced, under a part of the pons, and under the transverse band placed behind, which we have seen to be partly the origin of the facial nerve, as far as a longitudinal bundle which runs along the outer side of the corpora olivaria.

We have confirmed these two observations; and in repeating the second in several animals, we have ascertained that it is found neither in the ape, nor in several of the carniverous animals in which the nerves go off as in man; but always because the pons varolii in them also is broad. With regard to the first, it has appeared to us to be so certain, that we cannot help saying, that Vicq-d'Azyr has been deceived in deriving the roots of the fifth pair from the pedunculi cerebelli.* In examining always with the knife, he must have divided them, and lost sight of them too soon.

Every one knows that the *nervus patheticus*, or the fourth pair, arises transversely from the valvula Vieussenii, behind the *testes.* There is nothing which could make it be derived from the great medullary mass of the brain.

The *motor oculi*, or third pair, comes from the pedunculus cerebri, towards its internal edge, where it touches the *perforated cineritious space*, lying between the two pedunculi and the two processus mamillares, and receives some filaments from it. In man its roots are arranged in a line, nearly in the direction of the pedunculi, and the posterior roots are the longest, judging even by the external appearance; they proceed then rather from the posterior than the anterior part; but if a slight cut be made into the peduncle, this fact becomes still clearer. The greater part of these roots may be followed as far as under the pons varolii. A part of them is lost, or rather is formed around the black coloured part of the pedunculi. This arrangement is very well represented by Vicq-d'Azyr, *pl. xxxi. fig.* 2.

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* Mem. de l'Acad. des Sciences, 1781. p. 565.

In other animals the roots are more transverse, and more perpendicular; such, at least, we have observed them to be in the horse and the sheep.

The *optic nerve* is very generally considered as proceeding from the thalami optici; because its roots are spread out upon them in the manner of a thin membranous expansion, which almost entirely covers them.. However, anatomists have not been wanting, who supposed that they could trace at least a great part of these, as far as the *nates;* such as Morgagni, Winslow, Zinn. Santorini* describes this origin very carefully, and adds another, which he makes to come from the testes; his pupil and editor, Girardi, confirms it.† Vicq-d'Azyr, who was also very well acquainted with these connections of the optic nerves with the tubercula quadrigemina, pretends, however, that they have likewise other roots in the substance of the thalami, which, in the form of innumerable filaments, join the nerve in a great part of the course which it takes in surrounding the crus cerebri; and he boasted of this discovery.‡ But this appears to us to be a deception, into which he may have been led by his mode of making parallel incisions. It is very true, that a number of white filaments arise from the cineritious substance of the thalami; but they do not appear to us to go to the optic nerve. On the contrary, they are added to the bundle which comes from the pyramidal eminences, as we shall mention presently. Drs. Gall and Spurzheim have found out a mode of dissection which demonstrates this very well, and which we shall mention again.

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* Observat. Anatom. p. 63.

† Septemdec. tab. p. 34.

‡ Acad. de Sc. 1783. p. 529.

They suppose then that it is possible, at least in several animals, to remove from above the thalami, without affecting them, the medullary expansion of the roots of the optic nerves, and to follow them as far as the internal part of the nates, where they form a white lamina which occupies the centre of these tubercles.

The last point is certain; with respect to the first, as it can only be performed with the assistance of the handle of the scalpel, it is liable to the same doubt as all similar operations which may be tried on the brain.

Our anatomists farther remark, that in those individuals in which the optic nerve of one side is weakened and thinner, the corresponding tubercle is also smaller; and that in those species which have large optic nerves, the nates are of larger size; but that frequently in these cases the thalami are smaller.

The medullary tract which comes from the nates, meets in its course the tubercle called *corpus geniculatum externum.*

That which comes from the *testes* divides the first at the *corpus geniculatum internum*, and seems to slip under it, to cross it in passing forwards: thus Drs. Gall and Spurzheim do not believe that it belongs to the optic nerve; they have even for a long time thought that it forms the external root of the *olfactory*, which is indeed very nearly in that direction; but they have never been able to trace its continuity.

One of our number has made several researches on this part of the brain, which appears to have been only well known to Santorini and Vicq-d'Azyr, but which has never been properly delineated.

It is a fact, that in all quadrupeds, the principal bundle of the optic nerve goes from the nates to the *corpus geniculatum externum.*

It is also certain, that another bundle comes from the testes, which makes an angle with the first; and which, after swelling out to form the *corpus geniculatum internum*, seems to pass under the first bundle, and proceed further, but is soon lost to the eye and the knife.

Lastly, It is ascertained, that both the *testis* and the *corpus geniculatum internum*, are much larger in the carnivorous than in other animals; and this fact would be favourable to the idea, that they contribute to form the olfactory nerve, which is so conspicuous in that class.

But we think that we have seen in the ape, that the *corpus geniculatum internum* receives a bundle from the *nates*, as well as from the *testes*, and forms by their re-union a root of the nerve, which joins only very low down that which comes as usual from the nates above the *thalamus opticus*. Drs. Gall and Spurzheim have themselves besides remarked, what one of us pointed out long ago, that the dolphins and porpoises, which have no olfactory nerve, have, however, very considerable *testes*.

These animals, likewise, have the corpora striata as the others, which shows that these bodies do not possess the function attributed to them, of forming the olfactory nerve.

The first pair, then, will be the only one, the roots of which cannot yet be traced to the medulla oblongata, and which does not agree as yet clearly with the rule established in the memoir which we are examining.

Dr. Gall explains, conformably to the law mentioned in his first article, the enlargement of the optic nerves below their conjunction, by numerous filaments sent to them by the cineritious lamina interposed at the fore part of that conjunction; which filaments have been accurately described, and carefully delineated, by Vicq-d'Azyr.*

A strong objection was made to the origin, which our anatomists assign to the optic nerve, drawn from the structure of birds, which, it was said, have no *nates*, although their eye and optic nerve are of a very large size; but their answer is

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* *Mem. de l'Acad.* 1788. p. 548. et pl. xiii. fig. 1 et 2; and in his large work, pl. xxi. in all the figures.

The white mass then becomes stronger, and divides into a great number of diverging pillars, which constitute the white streaks of the middle of the corpora striata; the cineritious matter of the superior surface of these bodies gives also a great number of small filaments, as the thalami had given; at last, all these fibres are dispersed in the medullary mass of the hemispheres, where we shall follow them presently.

The two transverse whitish arches which are seen in the horizontal cut, and part of which Vicq-d'Azyr has expressed in his plate, are the places to which the greater number of filaments come from the superior parts of the thalami and corpora striata.

Such is the faithful description of what is perceived by the eye; one of us has delineated all this arrangement in man, quadrupeds, and birds, in which the essential points are nearly the same.

We are aware that there is no better reason for saying that the great fibrous bundles go from the pyramids to the hemispheres, than from the hemispheres to the pyramids; since the nervous influence proceeds in both directions.

But one may and ought to inquire which direction the small fibres of the thalami and corpora striata take. Are they supplied by these tubercles to enlarge the great medullary bundle, or are they given off from the medullary bundle to sink into these tubercles? This latter opinion certainly could have no probability, and no one will condemn Drs. Gall and Spurzheim for adopting the opposite opinion.

They would then be in the right in this sense, when they say that the medullary bundles go on always enlarging from the pyramids to the hemispheres.

But whence do the inferior extremities of the bundles, these same pyramidal eminences, come; or, whither do they go?

They cross each other about two fingers breadth behind the pons varolii, and immediately disappear behind that point, losing themselves on both sides in the two cords composing the inferior surface of the spinal marrow.

This is one of the most interesting facts in physiology and pathology.

Every body knows how frequent it is to see a paralysis of one side, occasioned by an injury of the opposite side of the brain; physicians of every age have endeavoured to explain this fact by a decussation which they vaguely supposed in the fibres of the brain, or in the deepest roots of the nerves.*

However, we see, almost every where, only transverse fibres, commissures, and not crossed fibres.

There is only one place, at the posterior extremity of the medulla oblongata, which presents a true decussation, which was discovered by Domenico Mistichelli, and very well described by him in 1709.† François Pourfour du Petit‡ likewise described it the following year, and was the first who made it known in France.

How has it happened that a point of structure so evident, adopted by Winslow,§ Lieutaud,‖ Portal, distinctly described,¶ and plainly delineated** by Santorini, should have been doubted by the great Haller,†† recently denied by very skilful men, and confounded by others, among whom may be reckoned Vicq-d'Azyr himself, with that of the transverse fibres which re-unite, in their whole length, the lateral parts of the medulla oblongata?

It is probably owing to the want of a sufficiently clear description, and perhaps, also, because the place of the decussation must be often cut in separating the head from the body.

It will be impossible to be mistaken in it, after the demonstrations of Drs. Gall and Spurzheim. When we separate from each other the two inferior cords of the medulla oblongata and spinal marrow, we see that they are separated by a pretty deep fissure, the bottom of which is occupied by transverse medullary filaments. This fissure is only interrupted at one place, which is what now engages our attention, and which is only two or three lines in length. The fibres of the pyramidal eminence of one side form there three or four filaments, which decussate upon the fissure with the opposite filaments, as the hairs of a mat, and which are blended afterwards with the rest of the medullary cord, into which they thus enter obliquely.

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* Arétée de Caus. et Sig. Morb. lib. i. cap. 7. p. 34. B. edit. Lugd. Bat. 1731. Nervi ab initio enati protinus ad oppositos transeunt, se invicem permutantes in figuram literæ X.

† Trattato dell Apoplessia, Roma, 1709, in 4to.

‡ Lettre d'un Medicin des Hopitaux du Roi, p. 12. Namur, 1710, 4to.

§ Traité de la Tête, No. 110.

‖ Anatomie Historique et Pratique, t. i. p. 591.

¶ Santor. Observ. Anatom. p. 61. xii.

** Ibid. § xvii. tab. II.

†† Phys. t. iv. p.

This decussation strikes the eye when we separate gently the edges of the longitudinal fissure of the medulla oblongata, because it is the only place where we cannot perceive the bottom of the fissure.

There is certainly some merit in having renewed the general knowledge of an important point of doctrine, which the doubt or denial of able men had caused to fall into oblivion.

Dr. Gall having established, as it appears, from this progression of medullary bundles of the brain across the pons varolii, the thalami, and corpora striata, his law of the increase of the medullary fibres by the cineritious substance, wishes to apply it to the cerebellum.

He has recourse here to the cineritious body of so singular a figure, which is found in the substance of the crura cerebelli, which has been called *corpus ciliare*, or *corpus fimbriatum;* the bundle called *processus cerebelli ad medullam* would give rise to the cerebellum, after having been reinforced by the corpus fimbriatum, as the pedunculi cerebri are by the thalami optici, and the cineritious part of the corpora striata. But perhaps this analogy is not complete. The corpus fimbriatum is enveloped, and, as it were, immersed in the medullary matter, instead of giving a passage to it; and we do not observe that it supplies it with any filament.

Some person will perhaps add, after the example of Vicq-d'Azyr,* that the brute animals have no corpus fimbriatum; but the truth is, that it is only of a smaller size; and as their cerebellum is also much less, the fact would be more in favour than against the idea of our anatomists.

Articles fifth, sixth, and seventh should be examined together. These three form the most particular points of the doctrine of Drs. Gall and Spurzheim; the seventh especially, relating to the possibility of unfolding the brain like a membrane, has made the most noise in the world; but, as is too commonly the case, almost none of those who have spoken of it have properly understood our authors; and those who have thought that they had found the fact in more ancient authors, had still less understood the circumstance itself, and the passages where they thought they saw it expressed.

The terms in which we have expressed the ideas of Drs. Gall and Spurzheim, will make you already perceive that the question is not the unfolding the whole brain; they have expressly stated, at the meetings which we have had with them, that the parietes of the ventricles are such as they appear to be, and that they do not conceal any folds, except behind, towards the plaited margin, where their folds were, a long time ago, known to, and delineated by, Vicq-d'Azyr; only, our anatomists say these thick sides, formed by the converging fibres, are the only bands which retain the folds of the external substance; this is attached there as the folds of a plaiting, for example, are fixed to the stuff of a gown; if you remove the principal stuff, the folds will extend and form likewise a plain piece of stuff.

Your committee have examined, with all possible attention, the hemispheres of the brain, in order to judge of the truth of so new a doctrine.

They think that they can, in fact, distinguish two orders of fibres in the medullary matter, but they find it necessary still to confine very much the idea which might be formed of the unfolding, according to the expression which we have just stated.

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* Acad. des Sciences, 1783, p. 471.

We shall now explain, in succession, these two propositions.

When we trace with the knife the fibres which come from the crura cerebri across the thalami optici and corpora striata, we see that they intersect, at angles more or less acute, those which go towards the middle line, and which form the corpus callosum and the fornix; it is even very easy to demonstrate their decussation in the inferior horn of the lateral ventricles; and the diverging fibres which come from the corpora striata, appear very evidently to form a layer external to the converging fibres which compose the corpus callosum.

But, does this external layer follow all the duplicatures of the still more external layer of cineritious matter, which is called cortical,—and does it unfold itself as the latter might be unfolded, if it were alone, and freed from all the white matter which fills it?

The one of these questions is evidently entirely independent of the other, and to be decided entirely by the testimony of the senses.

Taking, in the first place, the circumstance in the strict acceptation in which it seemed to be announced, we have used every effort to enable us to adopt or reject it with certainty; it would not be easy for us to make those who have not tried it understand what very great difficulties we have had to encounter. The medullary matter which fills the circumvolutions of the brain is so soft that it sinks down by its own weight; however lightly one supports with the finger the convexity or the dorsum of one of these circumvolutions, its two sides separate horizontally, and each of them carry away a part of the medullary matter which filled up the interspace. The vessels are not ruptured, because they are for the most part placed in the direction in which the rupture takes place; and, besides, because they pass through this medullary matter on account of its softness, as threads would pass through jelly or pomatum. It appeared to us, then, impossible to prove that there was an actual solution of continuity; on the contrary, both with the naked eye and the magnifying glass, the two laminæ of the white matter appeared covered with small prominent points, with small filaments, which had quite the appearance of as many lacerations. We even tried to make the laceration begin so as to leave a thicker lamina of white matter on one side than the other; and it appeared to us that the separation was made as easily as in the middle.

The argument which the authors of the memoir draw from the example of *hydrocephalous patients* did not appear to us much more conclusive. An accumulation of fluid in the ventricles of the brain may slowly distend the parietes, efface the projection of the circumvolutions, and render thin the medullary matter which surrounds them, without the latter being necessarily unfolded; the dropsy of the kidney distends and lessens the substance of that organ to such a degree, as to make it resemble a membrane, without any one being tempted to suppose that it was unfolded. The phenomena of hydrocephalous patients, who, having preserved their intellectual faculties for a long time, proves nothing further; for, as we do not know with what part of the brain, nor with what circumstance of its organization these faculties are connected, we can draw no conclusion from it relative to the essential structure of the brain.

Farther, we have also examined hydrocephalous patients; the parietes of the ventricles, though extended, had the same appearance as usual, and the circumvolutions, although thinned and partly effaced, nevertheless preserved their internal solidity.

Such were the ideas excited in us by the first memoir of Drs. Gall and Spurzheim, compared with the objects themselves; but these anatomists have since transmitted to us an additional note, in which they point out new modes of ascertaining the facts, and in which they explain their mode of viewing them with more precision.

M. Spurzheim has repeated in our presence these new experiments; vertical slices of the circumvolutions, macerated in nitric acid, diluted with rectified alcohol, became hard, and were divided more easily at the middle line; the same took place when they were boiled for 12 or 15 minutes in oil; when one blows on such a slice, or directs a small stream of water on it with a syringe, the separation may be made very easily in the middle, and scarcely at all at the sides. In the latter case, especially, the two surfaces which are separated, remain smooth, and the vessels passing through them untouched, without showing any traces of fibres which might have gone from the one side to the other.

These facts are accurate, but perhaps they only prove that there is less cohesion in the middle of a circumvolution than in the rest of its capacity, and not that it is formed of two laminæ simply applied, and not adhering together.

In other words, it may be admitted, in our opinion, that the white or intermediate portion of each circumvolution, is formed of two parts, which adhere to each other more slightly than the particles of each among themselves; or the union of them may be compared, for example, to that of the two laminæ of the dura mater, but not as was supposed, to that of the two sides of an intestine compressed, except, however, that the medium of union is not cellular matter, as in the dura mater; but the medullary substance itself a little softened.

Besides, as this is a point of fact, admitting an appeal to the senses, we do not pretend to give to our opinion more authority than it ought to have; this question cannot be long of being examined by every anatomist, and will find as many judges as observers; it cannot fail then of being soon finally fixed.

It is a great deal more difficult to demonstrate these orders of fibres in the cerebellum, than in the brain; and it is by analogy, rather than by actual knowledge, that Drs. Gall and Spurzheim admit their existence there.

With regard to what they say of the commissures of the brain, and cerebellum, their ideas are not new, or different from what has been already advanced by a very great number of anatomists; we may also add, that there is nothing in them which is not very probable.

We find the same probability in the commissures which *article eighth* attributes to each pair of nerves. It amounts almost to certainty in all the spinal nerves, whose commissures are the transverse filaments of the spinal marrow. We may suppose, that the small band which unites the two facial and the two auditory nerves in animals, is concealed in man by the pons varolii; the two pathetics touch each other on the valvula Vieussenii; the two optic nerves, as every one knows, appear almost to be intermingled together anteriorly to the pituitary gland; besides, their roots must unite at the same time as the nates and testes on the aqueduct of Sylvius. There would then only remain the abductors, motores oculorum, and olfactory nerves, which would not have visible commissures. The anterior commissure of the brain also, is evidently united to the olfactory nerves in brute animals.

This generality of the commissures seems to assist in explaining the unity of action of the double organs.

The *ninth article* is one of those which have been the most contested by the anatomists of Germany, and which is in fact the most susceptible of being so. It establishes, in the first place, the generality of tubercles of cineritious matter for each pair of nerves; secondly, the analogy of these tubercles with those called ganglions; lastly, the analogy of these two kind of organs, both with the cortical matter of the brain, and with the mucous expansions of the organs of the senses.

That each pair of nerves belongs originally to some tubercle, or, at least, to some portion of cineritious matter of some form, may be very readily maintained, with regard to the spinal nerves, and as far up as the par vagum, since cineritious matter extends the whole length of the spinal marrow, although it is not possible to follow the roots of the nerves so far; it is even certain, with respect to the auditory nerve which arises from the small cineritious band in man, or from the much more marked tubercle which supplies its place in the greater number of brute animals; and with regard to the optic nerve, which has at least two of these tubercles, the natis, and the *corpus geniculatum externum*, and perhaps also two others, the testis and *corpus geniculatum internum;* the olfactory has at least one at the place where it rests upon the cribriform lamella of the ethmoid bone; but the eye can perceive nothing of a similar appearance in the other cerebral nerves in man, in the mammalia, and in birds, although the par trigeminum has a tubercle in fishes.

The probability of the analogy of the spinal ganglia, and of those dispersed in the nervous system of organic life, with the portions of cineritious matter ascribed to the primitive origins of each pair of nerves, is liable to difficulties of a very different nature.

Anatomists, among whom it is sufficient to mention Winslow, have undoubtedly, for a very long time, regarded the ganglia as little brains, as sources of nervous action, independent of the great brain; others, such as Willis and Vieussens, have at least considered them as reservoirs of the animal spirits; or, like Lancisi, as organs which may be compared to a heart, and adapted to communicate a more rapid motion to these spirits.

Scarpa, of late, as well as Meckel and Zinn, in former times, was only willing to consider them as subdivisions, reunions, and re-compositions of nerves, enveloped and strengthened by cellular substance, bathed in a reddish fluid, and sometimes intermixed with fat.

The existence of this cellular membrane, and the fat which is sometimes deposited in them, have been acknowledged by the most distinguished anatomists of the present times. They are very distinctive characters, which do not permit the substance of the ganglia to be confounded with the cineritious matter of the brain. However, this substance has also some peculiarities, which ought not to allow it to be confounded with the ordinary cellular membrane; but the nature of these is certainly unknown.

The idea, that the ganglia dispersed between the different branches of the sympathetic nerves, have the effect of protecting the filaments of the nerves, reserved for the organic life, from the influence of the animal life, ought to occur, and, in fact, did early occur to physiologists; but why have not the spinal ganglia, which resemble the others so much, the same effect? Of this point we are still ignorant; all is here involved in darkness and obscurity.

It is of no service to science, to offer any new opinion, or revive any old opinion, without having more proofs in support of the one than of the other. It is better openly to confess our ignorance, and to separate distinctly the things which are known from those which are not. The human mind, it is said, supports doubts with difficulty; but it is precisely on that account, that the learning to bear with them ought to be one of the principal studies of men of true learning. The works of some modern physiologists have led to this short digression.

The analogy of the cineritious cortical part of the brain, and cerebellum, with the tubercles in its internal substance, such as the corpora striata, the thalami optici, the nates, &c. is much better established than that of the ganglia. Every one recognizes there nearly identity of substance; sameness of function would there also be readily admitted. But what can we say of the comparison between it and the mucous body which lines the skin, and all its internal prolongations? Here there can only be, with respect to structure, texture, in short, physical nature, a resemblance purely hypothetical. In absence of actual observation, it would require, then, to justify this comparison, some resemblance in the functions, in the uses, in the mode of existence during life; but where shall we find these?

We confess, also, that we do not comprehend the relation between these masses of cineritious matter, where the medullary bundles are strengthened in passing through them, and the rings which surround the base of the young branches of a tree: in trees, the branches go off in succession, one from the other; but in the nervous system, the whole is formed at once. It is impossible to find here any thing but an accidental resemblance.

Such, gentlemen, is the report which we have thought it our duty to make to you.

We have repeated all the observations of Drs. Gall and Spurzheim; we have even submitted to a new examination, part of those which were made by older authors, but connected with theirs; in fine, we have pointed out the degree of justice which we have found in the old as well as in the new remarks.

We, therefore, think that we have fulfilled, as far as lay in our power, the commission with which the class has honoured us.

You will now perceive, that we are far from adopting all the views and all the observations related in the memoir of these anatomists, but that we are also far from rejecting them entirely.

Finally, it appears to us, 1st, That Drs. Gall and Spurzheim have the merit, not of having discovered, but of having recalled to the attention of physiologists, the continuity of the fibres which extend from the medulla oblongata into the hemispheres, and into the cerebellum, which Vieussens first detailed, and the decussation of the filaments of the pyramidal eminences described by Mistichelli, by Francis Petit, and by Santorini, but with regard to which, some doubt had been entertained.

2d. That they are the first who have distinguished the two orders of fibres, of which the medullary matter of the hemispheres appears to be composed; the one of which diverges from the pedunculi, while the other converges towards the commissures.

3d. That, by uniting their observations with those of their predecessors, they have made it very probable, that the nerves called cerebral ascend from the medulla oblongata, and do not descend from the brain; and that, in general, they have very much weakened, not to say overturned, the system which makes all the nerves come originally from the brain.

But it also appears to us, 1st, That they have generalized rather in an inconsiderate manner the resemblance of structure, and of functions in the various cineritious, or ash-coloured masses which are met with in the different parts of the nervous system.

2d. That the idea which they entertain of a solution of continuity in the middle of the medullary matter of each circumvolution, which would permit of its being unfolded like a pipe or a purse, requires to be expressed in more definite terms than they have hitherto done, so as to indicate, that there is no complete proof of an absolute solution of continuity, but only of a more feeble cohesion.

We ought, however, to remark, that these two articles do not affect their general result, with regard to the kind of separation and of reserve into which they place the brain; and we ought, at the same time, to allow physiologists and pathologists to judge how far this kind of separation or laying aside, which anatomy seems to point out, is justified by facts, and how far it may favour the explanation of the numerous and astonishing phenomena of organic and animal life, and especially of those in which these seem sometimes dependent upon, sometimes distinct from each other.

The entering into all these questions would engage us in endless discussions, and foreign to our commission.

Nor shall we propose to the class to decide upon the conclusion drawn by our anatomists, that there is no circumscribed place in the brain to which all the sensations go, and from which all the voluntary motions issue, but that both of these functions may be exercised in a greater or smaller extent of the nervous system.

This opinion is undoubtedly that of Haller, Bonetus, and of the greater number of physiologists. There can be no doubt that it is from having confounded the metaphysical simplicity of the soul with the physical simplicity attributed to atoms, that the desire has originated of placing the seat of the soul in an atom; but the connection of the soul and body being necessarily incomprehensible to our minds, the more or less limited sphere which it is wished to give to the sensorium would not, in the smallest degree, assist our conception of it.

But all these subjects are also too foreign to the business of the class; they are too slightly connected with physical facts; they lead to discussions too vague to form a proper object of attention for such a body as ours.

We think ourselves, however, obliged to finish our investigation by remarking, that, if we were even to adopt the greater number of the ideas of Drs. Gall and Spurzheim, we would still be far from knowing the relations, the uses, and the connections of all the parts of the brain.

So long as we have not even a well grounded conjecture with regard to the functions of the pituitary gland, of the infundibulum, of the mammillary eminences, of the cords passing from these eminences into the substance of the thalami, of the pineal gland, and of its pedunculi, we must apprehend, that any system whatever, with regard to the functions of the brain, is very incomplete, since it will not embrace these parts so numerous, so considerable, and so intimately connected with the whole of this important organ.

This is almost finishing with as much doubt, and as much uncertainty, as we began; but one can only require, on every subject, the degree of probability of which it admits; and the natural philosopher always performs his task very well, when he neither exaggerates, nor diminishes this probability, and when he fixes its degree with precision.

It is of importance to repeat once more, if it were only for the information of the public, that the anatomical questions with which we have been engaged in this report, have no immediate and necessary connection with the physiological doctrine taught by Dr. Gall, with regard to the functions and the influence of the relative size of the different parts of the brain, and that all that we have examined with respect to the structure of the encephalon might be equally true or false, without any conclusion being drawn for or against this doctrine, which can only be determined by quite different means.

Given at the Institute, the 15th April, 1808.

(Signed) Tenon, Portal, Sabatier, Pinel, Cuvier.

The class approves of this report, and adopts its conclusions.

Conform to the Extract,

(Signed) Cuvier, Perpetual Secretary.

From the Edinburgh Medical and Surgical Journal.

Cases of Diabetes, Consumption, &c. with Observations on the History and Treatment of Disease in general. By ROBERT WATT, *Member of the Faculty of Physicians and Surgeons, Glasgow.* 8vo. pp. 328. Paisley, 1808.

PHYSICIANS seem now to be gradually recovering from those delusions with which a false theory, more than mistaken or unguarded observation had beset them. Indeed, we have witnessed so many signs of the decline of the stimulating method of cure in the greater number of acute diseases, and in some chronic, that we have long since ventured to proclaim its downfal and disgrace. And we have seen enough to persuade us, that this approaching revolution will tend to the improvement of practical medicine.

The practice founded on the Brunonian doctrines of debility, excitement, and stimulus, has slain, we believe, in sincerity of heart, its thousands and ten thousands. This doctrine not only set all former observation and experience at defiance, but so strong a hold had it taken on the imagination of its votaries, that medicine appeared to them unsusceptible of farther improvement. We remember the horror and incredulity that was excited some years ago, by the evacuating system of Dr. Rush, and of some West-India practitioners, in the yellow fever; and how a celebrated German professor, of the stimulating school, shrugged up his shoulders, and prognosticated the certain death of a patient in our infirmary, labouring under typhus, when he heard his physician prescribe some necessary evacuations: But the practice of Dr. Rush was successful, and our condemned patient speedily recovered. The most obvious and most fatal error of the doctrine now alluded to, was, that every evacuation was necessarily and directly debilitating, and that strength and excitement could only be effected by stimuli. In fevers, therefore, all evacuations were dreaded; while wine, brandy, and opium were considered as sovereign remedies, and too often employed with little discrimination, and still less regard to the state of local symptoms and organic affections. This fashion has passed away; and we now see physicians prescribing topical and general bleedings, drenching their patients with cold water, and scouring them with jalap and calomel, without at least any increase of the mortality of our fever wards.

Our dread of debilitating by such evacuations has been greatly lessened by the example of foreign practitioners, and by the writings of some enlightened physicians of our own country, particularly of a Currie, Jackson, and Hamilton.

These revolutions in the practice of medicine are calculated indeed to excite in our minds no inconsiderable degree of scepticism, and almost tempt us to conclude, with some, that nature triumphs more than art in the cure of diseases, at different times so oppositely treated. The truth is, that in fevers, there is no practice invariably proper; the physician must be guided by symptoms and circumstances; sometimes he must moderate by an antiphlogistic treatment the dangerous re-action of the vascular system, and sometimes he must support the vital energy by cordials and stimulants; but, at all times, he will find it useful to remove sources of morbid irritation. We have no hesitation, therefore, in believing that physicians are in a much fairer way, as well as their patients, now that they have recovered from the delirium of Brunonian excitement.

In the treatment of chronic diseases also, we have heard less of beef steaks, wine, and tonics, than we used formerly to do, and much more of temperance, exercise, and evacuation. The valuable observations of Dr. Hamilton on the exhibition of purgative medicines, seemed to lay open new views of the pathology of some of those chronical diseases, which might lead to improved modes of practice.

Still, however, we were little prepared to anticipate such a system of depletion, and results so singular and unexpected as are exhibited in the very interesting volume now before us.

These, it is true, rest solely on the authority of Mr. Watt, and have yet to pass the ordeal of general experience. But the observations are such as could hardly lead to any mistake; and they appear to have been carefully noted, and faithfully related. At any rate, they are now before the public; and as they are much too interesting to be long overlooked or neglected, the experience of others will soon, we hope, enable the profession to decide on their general application. For the present, it shall be our sole business to lay a short abstract of Mr. Watt's observations before our readers, earnestly recommending them, however, not to be satisfied with this, but to consult the interesting original.

The exclusive use of an animal diet, as taught us by Dr. Rollo, has certainly done more for diabetic patients than any method of treatment heretofore tried. Some few have been entirely cured by this plan, and a greater number have been partially, and for a while, relieved. But, upon the whole, its success has fallen far short of the expectations which the first publication of Dr. Rollo's cases had raised. With Mr. Watt it had uniformly failed, even when patients could be confined altogether to animal food, to which they have often an unconquerable reluctance.

The method of treating diabetes, to the success of which our attention is now invoked, consists in a strict antiphlogistic regimen, abstinence, and repeated venesection. When we reflected on the symptoms and progress of this disease, on the debilitated and impoverished system of diabetic patients, the vast and consuming waste of the solid and fluid parts of their bodies, and the imperfect action of their digestive and assimilating organs, we were ready to declare, with Captain Meredith's physician, that unless we met with a criminal having the disease, no request or opinion could induce us to perform *blood-letting* in such a case.

But instead of those disastrous consequences which our preconceived notions would have led us to expect, we find, from the cases now before us, that the force of the system, and the actions of the different functions increased in proportion to the evacuations, and that by these the disease was speedily subdued. These evacuations of blood, however, were neither sparing nor ordinary, but such as some practitioners would hesitate to make, even in well marked cases of inflammation, such as many would deem sufficient in the most violent case of pneumonia. For one of Mr. Watt's patients was blooded to the extent of 108 ounces in less than two weeks; and the patient, so far from being exhausted or debilitated by this treatment, became stronger and more active under it, and the disease yielded, symptom after symptom, as the blood was evacuated. One of the most singular, least expected, but easiest appreciated effect of this detraction of the circulating fluid was, we think, the salutary change produced in the constitution of the blood itself. In inflammatory diseases, as we repeat our blood-lettings we expect the buffy coat to disappear, and the firmness, solidity, and cupped form of the coagulum to diminish more and more. But in these cases of diabetes, the very contrary of all this obtained. The blood first drawn, was thin, and without buffy coat; the coagulum was so soft and weak, that it could not support its own weight, and was torn by the slightest force; as the operation was repeated, the blood assumed more and more of the inflammatory character, and exhibited at last a thick buffy coat, a strong, firm and tenacious crassamentum, contracted on its upper surface to the size of a shilling. "The improved texture of the blood, as venesection was repeated, the increased hilarity of mind, and general renovation of the living functions, warrant the conclusion, that, however weakening it may be in particular circumstances, blood-letting is a most important mean of disburdening the system, and of preparing the way for the final re-establishment of health and vigour."

Under the title Diabetes, five cases are detailed at considerable length. The first, a well marked example of diabetes mellitus, seemed, for some time, much relieved by the use of animal food, and elixir of vitriol; but the patient died a few weeks after his return home.

The second case is that in which Mr. Watt first experienced the good effects of venesection. The patient was a labourer, aged 36. The diabetic symptoms supervened to cold, caught in the preceding spring. He came under Mr. Watt's care on the 1st September. The principal symptoms at this time were, insatiable thirst, with dry parched mouth; impaired appetite; gums spungy and ulcerated; foul and white tongue; heat, and soreness in the region of the kidneys, and along the course of the ureters, particularly before making water; want of venereal powers and desires; lowness of spirits; debility and emaciation; skin harsh and hide-bound; pulse 60, weak and quivering; calls to urine frequent and urgent; the quantity voided in twenty-four hours estimated from twelve to sixteen pints; in colour pale straw, and of a fragrant odour and sweet taste. He was ordered a dose of castor oil, and to confine himself to a diet of animal food. This was persevered in from the 1st to the 4th *instant;* but it did not agree with his stomach; he had vomited, and conceived great reluctance to his diet. The thirst was now more severe; the *urine thirteen pints*, and strength declining rapidly. Some mucus coughed up, was followed by a little blood; this circumstance, with the recollection that Captain Meredith, on the evening after blood-letting, "felt lighter, cooler, and more cheerful, and had less pain about the kidneys," determined Mr. Watt to bleed his patient; which he did to the amount of fourteen ounces.—September 5th. The symptoms seemed much relieved; *urine nine pounds and a half;* the crassamentum of the blood black and void of tenacity. Finding so much relief from the first bleeding, Mr Watt resolved on a second. Eighteen ounces were accordingly taken from the arm; pulse, before bleeding, 50; after bleeding, 48, and pretty firm. Ordered to keep as closely as possible to the animal diet, with a small quantity of bread to every meal; and, to obviate acidity, forty-eight grains of carbonate of ammonia, prescribed in pills, to be taken daily.

6th. Appetite better; slept soundly; thirst less; urine little more than *nine pints;* pain across the region of the kidneys abated; crassamentum of blood so void of tenacity, that it cannot be elevated with a probe above the surface of the serum. Pills and animal diet continued.

7th. Urine as yesterday, and still sweet to the taste; pulse 64, soft and regular; was blooded for the third time, and to the amount of eighteen ounces. A flannel shirt ordered.

8th. *Urine little more than seven pints,* still sweet; thinks himself better; thirst is less; gums more healthy, and teeth firmer; blood as the last; skin dry, harsh, and scaly. On this last account, antimonial powders were prescribed, one to be taken every second hour, till some effect be produced.

9th. Vomited after taking the second powder; what he vomited was very acid; he feels almost no thirst; his spirits are light, and his mind cheerful; urine little more than *five pints,* and has a pretty strong urinous smell, and *salt* taste.

Pulse 76; *blood-letting repeated to eighteen ounces;* on tying up the arm, the patient became faintish, and went to bed. An unusual warmth was diffused over the body, and soon after, a gentle perspiration broke on his temples, and some other parts; pulse fell to 70; the blood is a little sizy, and the crassamentum is more tenacious.

10th. Perspired a little during the night; the ulceration, purple margin, and sponginess of the gums, are quite gone; tongue clean and natural; pulse 70, firm and regular; *urine* *little more than four pints*, of a pretty high colour, saltish taste, and urinous smell.

11th. Not so well; urine rather more than seven pints; pulse 80, firm and regular. Carbonate of ammonia to be repeated.

12th. *Urine eight pints;* his appetite has been keen, and he takes more food than he can well digest; he is desired to eat less.

13th. Urine little more than *seven pints, somewhat sweet to the taste;* slight headach; tongue foul; pulse 80, weak, but regular; there is clearly an aggravation of the disease. Flattered by former success, Mr. Watt had again recourse to the lancet, and took from the arm, in a pretty full stream, *twenty-four ounces of blood.* This the patient bore well, and without feeling the least faintish; pulse, after the bleeding, 72, firm and regular.

14th. *Urine only four pints, and in taste, smell, and appearance, nearly natural.* "He walked in the afternoon, and was astonished to find a degree of strength and agility, to which, for many weeks, he had been a total stranger." Re-appearance of the powers of virility; pulse 74, firm and regular; blood drawn yesterday inflamed, cupped, and tenacious. "The most violent cases of pneumonia, or acute rheumatism, do not show the blood more sizy and morbid in its texture." Lime-water prescribed.

15th. *Urine three pints and a half:* no thirst. Continues better.

16th. Has had a gentle moisture over the body; the skin becoming soft and healthy, the old cuticle desquamating; gums healthy; strength greatly recovered; urine three pints and a half. At his own desire, 16 ounces of blood taken from his arm in a full stream; pulse, before bleeding, 72; after it, 70, firm and regular.

17th. Urine not quite *three pints*, has a very salt taste, and strong urinous smell; blood remarkably inflamed, buffy coat, thick, firm, and contracted.

20th. Strength gradually improving; was at work all day yesterday; in short, from the 14th instant, it appears from the reports, that he has rapidly convalesced. The diabetes appears cured; nor did he afterwards suffer any relapse. The reports of the case, which we have so far presented to our readers in a very abridged form, are carried on by Mr. Watt to the 4th February, 1808, when the patient continued well. The whole case is very minutely detailed, from first to last, by our author. We have only noted occasionally those circumstances which serve most strikingly to connect the recovery of the patient with the treatment pursued. No one, we apprehend, can fail to remark the connection between the changes in the quantity and qualities of the urine with the different blood-lettings; and few will be disposed to attribute much to the different medicines occasionally employed, as the carbonate of ammonia, the antimonial powders, laxatives, lime-water, and bitters. The diet, it is true, all along consisted, in a great part, of animal food, but with some allowance of vegetable aliment. In short, as Mr. Watt has remarked, "Venesection is certainly the most prominent feature in the treatment. The abstraction of one hundred and eight ounces of blood, in less than two weeks, must have had a decided effect. He experienced relief in his feelings every time, and even, during that profuse evacution, gained strength." Yet, again to use our author's words, "In this instance, venesection was employed in the most forbidding circumstances. The pulse was slow, feeble, and not altogether regular; his strength and spirits were almost gone; the lower extremities had been œdematous to the haunches, and were always cold and lifeless." He concludes, by stating the following general observations on the good effects of the evacuating treatment.

"Even the first bleeding produced a degree of hilarity, to which, for many weeks, he had been a total stranger. After the third, the pristine vigour of his mind was completely restored, and his feelings rendered more comfortable. After the fourth, the pulse rose to about 80, became firm and regular, and some degree of perspiration appeared on different parts of the body. Still, however, there was no very remarkable change produced on the urinary discharge. After the fourth, there was evidently a relapse. The fifth seemed to act like a charm. The recovery, after this, was instantaneous and striking. The painful sensation in his bowels left him; the powers of virility returned; the gums became sound; the skin soft and perspirable; the saliva, the urine, and the alvine discharge natural; in six days he returned to his work; in two months he was restored to his original strength."

The next case is that of a student, aged 23. For some time before coming under the care of Mr. Watt, he appears to have suffered much from dyspeptic symptoms, lowness of spirits, and inability of mental exertion. His urine increased in quantity, and very frequently voided. He complained also of uneasiness and pain in the region of the kidneys. His skin was hide-bound; the gums spongy and ulcerated; and he was feeble and reduced. Pulse 80, very weak, but regular. The quantity of urine was six pints, two pints of which yielded one ounce seven drachms of extract, of the consistence of honey, and saltish to the taste.

The reports of this singular case are also very fully detailed. We had prepared an abridgment of them, that we might preserve to our readers the regularity of a journal, and enable them to form some idea of the effects of the uncommon treatment followed by Mr. Watt; but we must content ourselves with the summary which Mr. Watt himself has given us, and refer for the reports to the original. It matters little, whether we allow this to be really a case of well marked diabetes or not; it is at least one of great general debility, with local irritation of the urinary system, successfully treated by depletion, and an antiphlogistic regimen,—one in which health and strength were restored, though about one hundred and fifty ounces of blood had been withdrawn from the circulation during the treatment.

"During the first period, which extended from the beginning of October till the 13th, the patient abstained from animal food, wine, spirits, and every thing of a heating or irritating nature.

The bowels were kept moderately open; diluents were freely used, and an opiate taken at bed-time. This practice was persisted in for eleven or twelve days, without any advantage. On the contrary, the number of times of voiding urine had increased from eighteen to twenty-six or twenty-seven. The pulse had become weak, and intermitting; he had lost three pounds of his weight, and his strength and spirits were declining rapidly. The quantity of urine varied from five to eight pints; at times it equalled the whole liquid ingesta, and afforded, by evaporation, a copious extract.

"In the second period, which extends from the 13th October till the 26th of the same month, the patient, as to meat and drink, was allowed to live pretty much in his own way; the bowels were kept moderately open, and the opiate was occasionally continued at bed-time. On the evening of the 15th, and morning of the 16th, the patient was persuaded to eat beef steaks, and found all the symptoms greatly aggravated. The four succeeding days he was very abstemious, and every thing seemed to wear a more favourable aspect. On the 20th his appetite became keen, and a second indulgence produced a second relapse. On the 21st the emetic had a good effect, but, on the 23d, his appetite returned, and it was thought hard to deprive a person of nourishment, when his body was sinking so rapidly, and when the depletion had been so considerable. He was bled six times, and had lost above an hundred ounces of blood. But notwithstanding the continuance of some of the most distressing symptoms, the improvement, in this period, was very conspicuous. The pulse, from being weak and intermitting, had become firm and regular; the inflammation of the eyes was gone, and his vision had become strong and distinct; the blood, which he used to pass with his urine, had left him, and the urine itself was diminished in quantity, and more natural in its appearance. But the most striking improvement was the restoration of the mind to its wonted energy.

"In the third period, from the 27th October till the 12th

November, his health and strength were rapidly on the decline. Various injections were tried to allay local irritation, but without any advantage. The perinæum was blistered repeatedly. At first this seemed to afford some relief; but after a more extensive trial, it was found unavailing. The anodyne was increased from thirty-five to sixty drops, and afterwards to an hundred; but even this very large dose produced but a transient respite from the pressure. In this period, the greater part of his diet was animal food; for the last seven days it was entirely so; but made no impression on the disease.

" The fourth period extends from the 12th November till the middle of December. In this interval the treatment was completely changed, and the progress towards recovery was uncommonly rapid. At the commencement, though the patient was greatly worse on the whole than he had been at all, yet, in some respects, he had not relapsed to his original condition. The mind continued serene, and even retained a considerable portion of energy; the blood was still inflamed, and of due consistency. In this period he was bled four times, and lost about fifty ounces of blood. Seven blisters, each of them nearly the size of the hand, were applied to the region of the kidneys, and a pretty rigid abstinence was observed. No medicine whatever was used till the 29th November, when he began the lime-water, which seemed to have a happy effect on the state of his stomach and bowels, and probably contributed to his final recovery."—p. 134—137.

The fourth case, a lady aged 57, bears in many respects a resemblance to this last. There were pain and weakness in the region of the kidneys, impaired strength, and dejection of spirits; increase of urine, and frequent uneasy micturition; thirst, and foulness of tongue; the gums spongy and ulcerated; the skin dry and harsh; the feet œdematous and cold, with vertigo, dyspnœa, and palpitation. She came under our author's care on the 26th March; and, by the evacuating plan, she was freed from all her complaints in the course of three weeks, and began to recover her flesh and strength.

"The treatment of this case was more simple than any of the former, and though the patient was considerably advanced in years, yet that circumstance seemed neither to hasten nor retard the cure. After the intestines were well evacuated with castor oil, the patient was bled, in the course of a week, to the amount of fifty-two ounces. The blood had nearly the same appearance as in the other two cases, but had not acquired such a degree of consistence before venesection was given up. She was blistered first over the one kidney and then over the other, and, lastly, on the head; from all of which she experienced relief, and bore their application with a degree of fortitude that I did not expect, considering the debilitated state of her mind."—p. 168—9.

The fifth and last case of diabetes stands chiefly as another record of the fatal nature of the disease, and as a proof of the conversion of diseases; for here the diabetes assumed at one time the character of dropsy, and afterwards a diarrhœa put a period to the life of the patient.

To the detail of these cases Mr. Watt has added many important and judicious observations, illustrative of the history of this singular disease, which our limits oblige us to pass over in this place.

Our author now proceeds to make some more general physiological and pathological remarks on the history and treatment of disease in general.

The functions of the lungs, it is observed, are twofold, to assimilate the new materials supplied by the digestive organs, and to preserve the blood in a healthy state. In health there must be a due balance between the digestive and assimilative organs. If this balance be disturbed, disease ensues. This loss of balance may arise either from an increased action of the lacteals, or from an impaired action of the lungs while the lacteals continue their usual functions. And, in either case, there being more chyle thrown upon the lungs than they can assimilate, it must remain an incumbrance upon the system, or be discharged by one or other of the excretories.

Another function of the lungs is to repair the blood deteriorated in circulating through the body. By passing through the lungs, the blood becomes a proper stimulus to the left side of the heart, to the aorta, and its branches. By circulating through the aortic system, it acquires an opposite property, which renders it stimulant to the right side of the heart and pulmonary arteries. Now, the destruction of this balance is another source of disease. If, by cold, or other causes of retardation, the blood is longer in its return to the right side of the heart, it becomes more than usually deteriorated, and that in proportion to the time it is absent from the lungs, which, therefore, have more work to do in order to restore it, and the operations of the pulmonary system are consequently thrown behind.

" We have thus pointed out two sources of disease; the one arising from cold, or any other means which can retard the circulation, the other from an inequality of action between the lacteals and the lungs. Disease is generally begun by the former and protracted by the latter. When the lungs, in repairing the blood already in the system, have more to do than they can well perform, they can ill bear the introduction of new materials.

" It has been observed, that the nervous and sanguiferous systems act reciprocally. The blood may be deteriorated, and yet support life in an imperfect manner. The vessels which increase and repair the solids may be in want of proper materials, though the system be overcharged with blood. The nervous system, being deprived of its natural support from these vessels, acquires a depraved sensibility, and being acted upon by an unnatural excitement, all the phenomena follow, which we have described, as attending a diseased habit. The greatest number of the excretories are idle from want of arterial blood, the only stimulus which can call them into action. The liver, receiving its stimulus from venous blood, has more to do than in health; hence the origin of bilious complaints, which with low spirits and prostration of strength, generally mark the first stage of disease."

The balance is restored by reaction.

"The power of reaction resides in the solids. The blood, by being long absent from the lungs, acquires a property which renders it stimulant to the right side of the heart, and to the pulmonary arteries. By their action, the blood is poured into the lungs, where it receives a new stimulus, and is sent to the left side of the heart, circulates through the body, and is again returned to the lungs. This rapidity of circulation, to make up for the time which has been lost, is fever. The oppression on the lungs, if remarkable, becomes dyspnœa or asthma.

"If the system possesses sufficient vigour, the reaction goes on to a proper crisis, that is, till the blood be restored and become the natural stimulus of the nervous system, the proper food for the excretories, and for the vessels which repair the solids. In place of fever, the balance is often restored by a critical evacuation. If the superfluous matter take to the intestines, it produces diarrhœa; if to the kidneys diabetes; if to the uterus menorrhagia; if to the cutaneous exhalents profuse perspiration. All these evacuations to a certain extent, are salutary. They often prevent fever, shorten its duration, or render it more mild. If the reaction fail to produce a salutary crisis the system falls back, collects new vigour, and resumes the conflict, as in intermittent fever and other periodical diseases. In other instances, such as hypochondriases, it repeats the same thing over again, or tries other means of relief, and is thus said to counterfeit every disease; that is, it employs many efforts to throw off the incumbrance, but is generally unequal to the task. After a longer or shorter struggle, a confirmed phthisis, diabetes, diarrhœa, dropsy, or some other disease terminates the patient's sufferings.

"In a great many instances, the practitioner has to contend with diseases of a different description. His object is not to excite reaction, but to render it more moderate. Inflammation arises from reaction coming on too suddenly. If a limb, after exposure to a great degree of cold, be suddenly warmed, inflammation, and perhaps even gangrene are the consequences. When a part of the body has been cold, and wanted, for a while, the stimulus of arterial blood, if the circulation be suddenly restored, it is apt to suffer the same fate. Inflammation, in the last instance, arises from the same cause as in the first; from the too sudden application of heat. If proper means had been used to repress reaction, the organization of the part might have been saved.

"The resistance resides in the fluids. If the action of cold, or any other cause which retards the circulation, be long continued, or, if the patient take too much food, the solids are overpowered, and no effective reaction can take place. The quantity of the blood is so far above the power of the lungs, that little or no arterial stimulus can be communicated to it. The system, after a longer or shorter struggle, gives up the contest, and the unhappy victim either dies, or drags out an existence more dreadful than even death.

"When reaction recurs from time to time, but is inadequate to restore the system, diseases are called chronic. When it comes on rapidly, or is too violent in its exertions, they are denominated acute."

There is considerable ingenuity, and, we apprehend, much fancy in this pathology, upon which, however, Mr. Watt endeavours to ground the explanation of his practice. The observations which follow under the title *practice*, are chiefly applicable to the treatment of chronic diseases—or to the general means of correcting diseased habit. And the principal means, according to Mr. Watt, are abstinence and evacuations. His remarks on diet, emetics, purgatives, diuretics, diaphoretics, and venesection, are connected with his peculiar pathology; but though general, they are important, in so far, especially, as they have been actually carried by him into effect, and with great advantage. Symptoms of seeming debility, coldness of the surface, faintness, small unequal pulse, furnish, according to Mr. Watt's view and experience, no objection to these evacuations, not even to repeated venesection. All these rapidly disappear, as the system is relieved from the resistance arising from the fluids. And venesection is held forth as one of the most powerful means of regulating the reaction of the system, whether this may be depressed or exalted.

"It is not sufficient," he observes, "that patients be bled till the more urgent symptoms are removed. After this, they often remain weak and languishing, subject to cold shiverings on the least exposure, with a universal tremor; pulse sometimes quick and full, oftener small and indistinct; tongue very white; a want of regularity in the excretions; generally one of them in excess, and the rest deficient: a want of sleep, with universal restlessness and anxiety. These symptoms are generally attributed to the loss of blood, and we are told that nothing can remove them, but time and nourishment. To prove the fallacy of these assertions it is only necessary to enjoin abstinence, and bleed the patient again. The relief obtained from venesection, in these circumstances, is not less striking than in any other stage of the disease. The blood is always inflamed, and, on cooling, exhibits a very thick buffy coat.

"If the evacuation be carried a sufficient length, the cold shiverings and tremors are removed; sleep is restored; the pulse becomes soft and regular; the excretories resume their healthy functions; the patient experiences great relief in his feelings, and an uncommon degree of mental serenity and cheerfulness succeeds."

Another remedy which Mr. Watt speaks highly of in the correction of diseased habit, and from which, he thinks, the most beneficial effects may be derived in the treatment of chronic diseases, is mercury. It acts as a stimulant to the arterial system and excretories, and excites and promotes reaction, while venesection, by diminishing the quantity of the fluids, obviates resistance.

"We are often told of the pernicious effects of mercury on the constitution; but if I were to judge from my own experience I would form an opposite conclusion. In cases where mercury was carried to such a length, that the patients have been for two weeks without tasting almost either meat or drink, the cure was most complete. In some instances this was done where the patients were supposed to have suffered greatly from previous salivations; and so far from injuring the constitution, the process appeared to give it new energy, and the most perfect health has been the consequence.

"Several years ago, I treated a person, above seventy, with a course of corrosive sublimate; the mouth in a short time was so much affected, that for fifteen days he neither ate nor drank; his body was much reduced, but he had a rapid and complete recovery, and has since enjoyed excellent health.

"The bad effects of mercury, like those of venesection, are to be attributed to not carrying the process a sufficient length at once. A cure from mercury is not to be expected, while the patient's body remains unreduced, and while he continues to take his usual diet. Venesection contributes to the former of these, and the sore mouth effectually secures the latter."

To these general observations, our author has added some cases and remarks on the history and treatment of asthma, cholera, colic, consumption, chorea, and plethora. The general scope of these histories and reflections is to show, that a cold stage, or interrupted balance between the circulating and pulmonic systems, always precedes the stage of reaction which properly constitutes the diseases, and to illustrate at once his general doctrine, and method of treatment in all of them. In the case of chorea and convulsions, the boy, aged four years and three months, had been vomited and purged smartly without benefit; purgatives too were continued throughout the cure; but two large bleedings from the jugular vein, the first to the amount of nine ounces, and the second, four days afterwards, to the amount of ten ounces, had evidently the merit of putting a stop to the violence of the disease. A few grains of calomel at last brought away a large lumbricus, and a number of very hard scybala.

"This case, 'it is observed,' seems to prove, that the convulsive motions do not depend on obstruction of the bowels, for they were completely removed, even while the hardened scybala remained. I am apt to think that the convulsive motions, the torpid intestines, the chilly state of body, and the peevish irritability of mind are merely symptoms of diseased habit. The bleeding had an instantaneous effect in relieving them; *after the system was repaired*, a few grains of calomel enabled the intestines to discharge scybala, on which dozes three or four times as large had no effect in the earlier part of the treatment."

We have now done with Mr. Watt, whose book contains many important observations and reflections. The practice, so different from any thing we have been hitherto conversant with, has surprised us much, not less on account of its seeming success, than of the persevering boldness with which it has been followed up in the face of every professional prejudice. Nothing could have carried Mr. Watt through with this, in the sick room of his patients, nor induced him to submit the whole to the public eye, but a conviction of its utility and importance. On subjects which can be decided by observations and experience alone, we forbear to make any further reflections; but we earnestly hope, that the subject started by Mr. Watt will neither be overlooked nor neglected.

REVIEW.

A View of the Nervous Temperament; being a practical Inquiry into the increasing Prevalence, Prevention, and Treatment of those Diseases commonly called Nervous, Bilious, Stomach and Liver Complaints, Indigestion, Low Spirits, Gout, &c. By THOMAS TROTTER, *M. D.* &c. pp. 338. *Troy, re-published*, 1808.

DR. TROTTER has undertaken a task in this work, which the most experienced physicians best know to be very arduous. No diseases embarrass our first steps in the profession so much as those of the nervous kind, nor continue to form more anomalous exceptions to our rules of practice. The clue which Dr. Trotter found most successful in leading him through this labyrinth, was the study of the nervous temperament. It results from predisposition, and this may be hereditary or acquired. By the *hereditary predisposition* he understands an original conformation of body, transmitted from the parent to the offspring; by reason of which, when particular exciting causes are applied, a similar train of morbid phenomena takes place. A predisposition may, therefore, appear long before any symptoms of actual disease has shown itself; as in the phthisically disposed, a person will be easily affected by weather and sudden changes of temperature, and, on slight occasions, liable to cough, hoarseness, tightness, or stitches of the breast.

> " The child born of nervous parents, that is to say, persons of weak digestive organs, and irritable nervous system, subject to bilious and spasmodic complaints, &c. will, at the breast, be very liable to bowel affections, such as cardialgia, flatulence, constipation, or diarrhœa, gripes, yellow gum or jaundice, &c. These will be apt to come on from slight occasions, as when the milk of the nurse is affected, either by her passions or impro-

prieties of diet, or when any thing has entered its food that is difficult of solution in the stomach. These causes will frequently operate with such effect, and to such a degree, as to induce convulsions and death.

"In some of the diseases of infancy, such a child will suffer more than other children. At the teething period, it will be more likely to be seized with bowel disorders and convulsions: in the eruptive fever of the small pox, it will be more prone to these fits: and in the hooping-cough, as being a spasmodic disease, the nervous infant will be a severe sufferer. Frights of all kinds, that ruffle the temper, and impatience under bodily pain, will be attended with irritable passions. Worms are the consequence of weak bowels and disordered digestion; a child of this description will, therefore, be much troubled with worms, and generally their most troublesome symptoms. These symptoms will sometimes put on all the appearance of a confirmed *hydrocephalus internus*, even to the last degree of strabismus. I have seen so many instances of recovery from this apparently hopeless state, that I am disposed to refer the whole to their intestinal vermin, or to some aggravated attack of stomach affection, depending on original nervous predisposition.

"At the age of *puberty*, if a female, it will be liable to be affected with that disease usually called *chlorosis*; which, besides so many symptoms of dyspepsia, combines with it the *emansio mensium*. The *chorea*, or St. Vitus's dance, also appears about this age: and in numerous cases which I have seen of both complaints, I am disposed to think, that they *never* occur without manifest predisposition, and are therefore to be considered rather as symptoms of the nervous temperament, than as distinct diseases. The changes which now take place in the constitution, conjoined to the quick growth of the body at the same time, will render this a most critical period. At this season, the nervous woman is first affected with hysterics; these added to many painful symptoms of increased irritability, will be apt to recur at *distinct intervals*, through life, particularly if unmarried. Extreme delicacy of stomach, dyspeptic affections, and what are called bilious, dysuria, leucorrhœa, hemicrania, &c. with other nervous signs, will be the lot of this hereditary predisposition.

"The female that is born of *gouty* parents, comes entirely within this description. The fact is notorious, that what is called *regular gout*, or gout showing itself in the extremities of the body, seldom attacks the fair sex: and when it happens, the woman is marked by a more masculine form, or other external signs indicating this peculiarity. But even this will not explain why the female is so seldom affected with arthritick inflammation. Something may perhaps be sought in the generative faculty: the *castratos* are said to be exempt from gout: regular gout is very rare before the age of puberty; and all the women whom I have known subject to inflammatory gout, except one, had never borne children. Gout, in all its shapes, is preceded by stomach affection; so also is the *period.* Again, the stomach recovers as the pain and inflammation fix in the joints; and if they prematurely recede, the affection of stomach returns. It is the same with the *period:* when one goes on properly, the other declines in due time: and if cold, passions of the mind, or other causes, bring on a sudden stoppage, all the complaints of the digestive powers instantly recur.

"But as the dyspeptic symptoms which attend gout are so much alike in both sexes, making allowance for the greater sensibility of the female, they strongly support the idea, that the chylopoietic viscera are the original seat of this disease; and these the primary symptoms of gouty diathesis. The child, therefore, who is born of arthritick parents, has in its constitution what may be called the predisposition to nervous and bilious diseases. In infancy it is prone to all stomach and bowel complaints from slight causes, as have been described, and these will be its attendants through life. Even the man of the gouty family will not be exempt from this disposition to be dyspeptic and bilious, on every kind of excess or improper indulgence. In both sexes, they show a stronger tendency, as they appear early; for that proves a weaker structure of the digestive powers, and greater debility of frame; just as gout is to be more dreaded in proportion to its attack at an early age. Thus the youngest votaries of Venus and Bacchus will run greater hazards of immature gout, and premature decrepitude as a consequence.

"2. The other division of predisposition to these diseases, is the *acquired predisposition;* or what may be brought on by causes which especially weaken the frame of nerves, and the chylopoietic organs. This predisposition may take root, even during the earliest stages of infancy, in children born of the healthiest parents. The effects of the milk of an unwholesome nurse often lay this foundation. It may happen where the child is not sufficiently nourished; where the nurse is much affected with the disorders herself: if she drinks too freely of spirituous or fermented liquors, or is in the habit of taking opium, or other drugs. Bad lodgings; impure air; hot rooms; chills from exposure to cold; washing the infant in water too cold or too hot; want of cleanliness; dosing it with hot things, whether spirits or aromatics; frequent opiates in any form to make it sleep; deficient exercise; sweetmeats; frequent recourse to medicines, such as emetics and purgatives, but particularly calomel: and if these articles are often repeated at any stage of childhood, without competently prescribed, they must infallibly debilitate the stomach and bowels, and induce the diseases in question. To all these causes may be added, the effect which the furious passions of some women have on the infant they suckle. But even children at an early age, are not beyond the reach of moral causes, particularly females, and these will often operate with great force on sensible minds.

"The girl whose health has been thus ruined, by improper nursing in infancy, or bad treatment in childhood, will be apt to suffer at *puberty*, and at the *change of life*. At the former period, violent hysteric and chlorotic affections will commence, and health will be precarious for a length of time. Such a woman will be generally liable to sterility from constitutional infirmities; to abortion in the early months, and to premature births: these will often happen from slight causes; and much danger will be encountered during parturition and in the puerperal state. Other diseases of equal delicacy, which are too often the bane of female life, will be the portion of such a woman. These girls acquire an inactive sedentary turn in early age, which is seldom overcome; they seem to vegetate, rather than live, and but rarely reach to fifty years. When women of this kind of habit suckle their own offspring, or that of others, they much injure the health of the infant as well as themselves: such children commonly die in great proportion under two years; and if they survive that age, it is with indelible marks of a puny and vitiated constitution. Among the poor in large towns, the proportion of deaths in infancy is great, chiefly owing to vicious nursing; and much caused by that nervous weakness brought on by the use of tea and spirituous liquors."

Dr. Trotter then gives the name of nervous temperament to the predisposition, whether it be hereditary or acquired, and considers it as a permanent state of the body, that can not be easily changed, and will commonly remain for life. It is to be observed in different shades and gradations mixed with the other temperaments, but where it is exquisitely formed, it is known by the following signs:

“ *A sensible, irritable, and mobile condition of nerves; by which different organs of the body, from slight causes, are urged into violent and involuntary action; and their motions and sympathy often reversed; giving birth to false perceptions and erroneous judgment; and sometimes accompanied with pain of the acutest kind.* This temperament is said to bear all evacuations ill, especially the loss of blood; and also is easily injured by medicines of the rougher class; it is not very liable to diseases of the inflammatory kind.*

“ It is evident, from the history of these diseases, that where the nervous temperament prevails, all the causes which operate upon it bring forth motions and sensations very different from what is found in a healthful structure of nerves, in persons who are without the predisposition. This is more exemplified in what may be called the mental causes, than in the corporeal. The moral evils of life are very much of a relative nature; their effects depend, in a great measure, on our capacity of feeling, for receiving them; or to the fortitude which we are able to oppose to them. Thus, one man is condemned for sinking under adversity, as a proof of deficient virtue and spirit; while another is extolled for his courage, as a token that he possesses nobleness of mind. Yet the physical trait of their temperaments will best decide with impartiality on their respective merits. The first may be a weak-nerved being, and a good man; and the other, under apparent resolution of soul, may possess nothing beyond want of feeling.

“ But this even applies to the corporeal causes. Let the two persons start together, to drink a bottle of ardent spirit in the twenty-four hours, for life. The nervous man will most likely find his frame shook to pieces at the end of two weeks, or as many months; while the other will continue his potation for as many years. When the first dies, of weak nerves and tuberculous liver, at the end of three months, the other may still remain strong and hale. It may now be said that the first was a drunkard, and died *felo de se:* but the survivor will have a chance of preserving his reputation for sobriety. Of so much importance is the study of temperament in judging of the causes of these diseases.

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* Gregorii med. ther. conspect. vol. ii. chap. xxiii.

"The living body possesses the faculty, if I may so call it, of receiving impressions, and retaining them, even to the hazard of its destruction. All predispositions are of this kind. When a person, subject to gout, undergoes a long mercurial course for the cure of syphilis, he may not be always warned of the increase of predisposition, which he will infallibly acquire by this process; though he may be duly told of the necessity for mercury to subdue the other disease. And if it were left to his choice, he would still prefer the antisyphilitic regimen, and make the best he could of his gouty diathesis. If, besides, he is attached to the bottle, or has contracted the habit of taking opium in large doses to ease his griping pains, he is going fast on to give the final blow to health; and the last degree of this, is the completion of predisposition, or constant gout and nervous affliction. So that *predisposition*, in its various stages, is the medium between health and disease. A gentleman subject to gout, weak nerves, and all their horrors, consulted me some time ago. He took his wine freely, and an opiate every night at bed-time. The last, he said, was to ease his spasms,, and to give him rest. He was also of extremely slow bowels. I warned him of the dangerous habit he had got into; and in order to save the remainder of his constitution, recommended him to give up his bottle and his laudanum immediately. But the conflict was too great for his fortitude; he did not call upon me a second time. This man, I dare say, was both able and willing to pay a physician, provided he could find one to his mind. That society must be undergoing the last degree of vitiation, where the faculty of medicine receives gold, and returns poison."

"The mind and body being connected by the *nervous system*, the same train of symptoms appear, when those causes are applied to the body, which first affect the chylopoietic viscera; such as a mercurial course, or a debauch with ardent spirit or opium. Now, what can be the reason, that these passions, or the articles just mentioned, have such extraordinary action on nervous people, while on others they have no such effect? I would explain the fact in this manner: the hereditary temperament is supposed to inherit all the bad impressions of its progenitor, hoarded as it were in the structure of its nerves: in like manner the acquired temperament retains, or records as it may be termed, all the effects of vicious indulgence. So that when any fresh gust of passion arises, or any luxurious stimulus is applied to any portion of the sympathetic nerve, these accumulate the quantum of predisposition; and a *nervous fit*, or a *bilious attack*, is the immediate consequence of every new trouble of mind, and of every recent debauch of the body. Thus the habit may become so completely nervous, or in other words, the *predisposition* may arrive at that height, that the faculties of the soul will be worn out, and fatuity take place; and the body will be so enervated as to be in a state of constant pain, tremour or convulsion. Such cases, in no small numbers, are certainly to be seen every day, by medical observers who possess discernment to appreciate experience.

" The operation of ardent spirit, which includes wine and all fermented liquors, and opium, if not all narcotics whatever, is much alike on the animal economy. They assail in the first place the nervous system, and all other effects are secondary. The nerves of the stomach feel the first injury: but so intimately are these nerves connected with the whole that supply the chylopoietic viscera, that they are instantly drawn into consent. The stomach, intestines, pancreas, liver, and ducts, become thickened, and grow torpid by long indulgence in these articles, and unequal to their functions. But it will not be doubted by any person who has attentively watched the effects of opium and ardent spirit, that all the early symptoms of indisposition which they create, are purely nervous, and extended by sympathy. I was formerly of opinion, that the enlarged liver was owing to the constringing power of the alkohol being spread from the duodenum to that viscus through the ducts. But this explanation is not satisfactory; and opium and other narcotics cannot act by hardening the fibre or animal solid. *The hepatic system must be injured by nervous communication and sympathy with the stomach.* But if it is common in a severe disease, and after long continuance, for the liver of dyspeptics to be found sometimes enlarged, where no vinous stimulus had been used, such a case must have been of nervous origin to a certainty; and from this we have a right to conclude, that the diseased liver of drunkards is almost always of this kind. When the hepatic nerves are rendered weak, mobile, or torpid, by the excessive use of alkohol, it necessarily follows that this debility is extended to the whole substance of the liver. There is no proof that an inflammatory stage and fever are always the consequence; on the contrary, I believe inflammation seldom happens. The hepatic system of inebriates does not appear injured till a very considerable degree of debility has taken place throughout the body: there is a general derangement before this assumes any signal mark of disease; and many of the worst symptoms are often present, when nothing beyond nervous affection can be suspected. Jaundice has too frequently been deemed a sign of enlarged liver: but it so happens that the jaundice of nervous people has little connection with the liver.* The irritable, or torpid state of the duodenum and ducts, just as the nervous power happens to be deranged in them at the time, is the most common cause of biliary obstruction, and consequent jaundice. Nay, the appearance of jaundice must often be a proof that the secreting office of the liver is perfect. During the inverted motion of the muscular fibres of the intestines, so peculiar to the nervous temperament, the opening of the duct that conveys the bile, from its singular form, must be very liable to obstruction. And this is thought to be the most common cause of icterus.

"But if the torpid and enlarged liver is thus proved to arise from nervous debility, must not the cure turn upon an invigorating plan? The first step to this must be to remove the causes which brought it on; in the drunkard, let the bottle be laid aside *entirely:* let such mental and corporeal stimuli be used as have a restorative quality; and health must return, if it can be brought about by human means.

"But these principles apply with still more force where biliary obstructions infest the female constitution. The irritable nervous system, and delicate bowels of most women, ought to make medical people consider well before they attempt violent remedies to overcome obstruction. The true method of cure in weakened females, must be to strengthen the whole chylopoietic viscera, by such medicines as communicate permanent energy to the nervous system: to obviate slow bowels or diarrhœa, to correct acidity and flatulence; to attend to the quality and quantity of the food; to regulate the passions; to observe strict rules of air and exercise; to avoid cautiously every excess in stimulation, whether mental or corporeal: in short, to *reverse all established habits, and to force a new train of actions upon the temperament.* I speak from much experience on this subject: and it is admitted by the advocates for the mercurial course, that they have no certainty of the *supposed liver affection* being removed; but their patients are at intervals returning to the medicine; and we have known some instances where ladies of the nervous temperament were *taken very unexpectedly out of their hands*, by what was called the cramp of the stomach; a symptom that sometimes follows with a quick pace the exhibition of mercury. These gentlemen must have often met with obstruction and suppression of urine among nervous patients; yet it does not appear that they consider calomel the best relief to the kidneys and bladder. In fact, in most cases, of both jaundice and ischuria, we are to look to nervous sympathy for their cause; for they commence and disappear in general so suddenly, that local causes can scarcely be suspected. We might as well dissect the top of the œsophagus for the cause of *globus hystericus*, as attempt to fix the pathology of these versatile movements in the secreting or other organs; or inspect the brain of a hypochondriack for the picture of his *blue devils*. The migratory power which these affections possess, of traversing every part of the body, is the inscrutable *idiosyncracy* of the NERVOUS TEMPERAMENT.

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* Within these four years, of rich and poor who have consulted me, not less than 50 cases of *supposed diseased liver* were of the number. Yet the treatment proved that no fixed hepatic affection was present. Many of these had their complaints aggravated by the previous use of mercury.

" A physician of our acquaintance for many years was subject to dyspepsia, and conceived that he had a *diseased liver*, as he termed it. For twenty years he took calomel in large doses very frequently; and all this time with a manifest increase of his complaint. The bowels however came to that torpid state, as to resist every common laxative; and he at last got to the enormous quantity of thirty grains of calomel for a dose. I speak from his own authority. He became weak and emaciated, and at times suffered great pain; and sunk under the debility, which was to a certainty produced by the mercurial poison; for when the body was opened, *no liver disease appeared!*"

The author sums up his opinion by concluding, that the pathology of these diseases is to be sought in the deranged sensations, and inverted sympathies of the great sympathetic nerve; and in the irregular action of all those organs to which it is distributed.

The causes therefore, whether moral or physical, exert their influence on this portion of the nervous system; whose office directs the most important operations in the animal economy, and binds together, in one great circle of feeling, actions and motions, both distant and opposite. Hence a concourse of symptoms, of the most extraordinary kind, that invert the usual functions of so many viscera; suspend their powers, or give to them new movements; by which means a train of false perceptions occupies the mind; and ideas the most monstrous and incongruous supplant, for a while, all rational thought. In this reciprocal action between body and mind, in whatever part of the circle disease commences, it is quickly communicated to all the others. For as bowel complaints speedily affect the mind and depress the spirits, so all violent emotions, in their turn, induce affections of the chylopoietic viscera, and raise such commotion throughout the sensitive system, as to bestow the nervous character on these diseases.

The author does not enter so directly on his subject as the view we give of it here. His plan is to give first a representation of the health of the savage state, in order that the contrast with civilized life may appear more striking. On this we must be permitted to observe, that however well qualified an European physician is for making correct and relevant remarks on the diseases that offer themselves to his observation, when he reasons from a state of life and manners with which he is not conversant, however we may admire his fancy and his erudition, we can not rely on his conclusions. Dr. Trotter, like other Europeans, in giving a view of the savage state, quotes *Tacitus de moribus Germanorum;* but for us, we have more authentic sources of information; we have the savages themselves living in our vicinity. Their manners and condition of life are frequently before our eyes, and, upon the whole, we do not find that they possess any advantage in point of health over their civilized neighbours. We admit indeed, that these lead a hardy life, and enjoy universally the benefits of wholesome food, comfortable habitations, and good clothing, from all which we find that their health is better, their constitutions stronger, and their increase greater. The diseases of the savage are rheumatism and consumption, and they afflict him severely. His wife brings forth at intervals only of three or four years, and three or four children break her constitution.

Our author confounds the state of the savage, which is in no respect desirable, with that of the countryman who ranges freely in the open air, leading a life of moderate labour, possessed of all that is necessary to comfortable subsistence, and unused to enervating indulgences or excess. This we will readily confess is the state of life most favourable to health; it is universally that of our happy country; it is the one that contrasts most usefully with the effeminacy of polished manners, and that to which we would wish to bring back the nervous frame and surfeited constitution. This is feasible; but a scheme of reform that would recommend the scanty clothing, the smoaky hut, the stinted food of the poor savage, must be founded on a limited acquaintance with the real circumstances of his condition. It is, indeed, deeply to be regretted, that too much refinement and luxurious indulgence bring on a train of nervous ailments, which are too great a price for the ease and abundance that precede them; but the remedy should be sought for in active occupations, in the open air, in such wholesome employment as occupies a farmer's wife in the country. A lady, however refined, who submitted in part, at least, to this salutary exercise, would no less than the savage, find the state of pregnancy easily borne, and the evolutions of the birth not slower than is proper.

From a view of the savage state, the author proceeds to give a medical description of the inhabitants of a town or city; being an analysis of society. He divides those into the following classes, and gives a concise history of each. 1. Literary men. 2. Men of business. 3. The idle and dissipated. 4. The artificer and manufacturer. 5. Those employed in drudgery. 6. Persons returned from the colonies. 7. The female sex, consisting of the higher, middling, and lower orders of women. The sedentary life of men of letters, the close application of men of business, the debaucheries of the idle and dissipated, the unwholesome occupations of the artificer and manufacturer, the exhaustion of habit of those who return with fortunes from the colonies, the late hours and debilitating indulgencies of the female sex, are all justly enumerated as causes of the nervous temperament; but some of them are such as operate very little in our state of society, and others not at all. We deem it important to point out this difference to the American reader, of the work under consideration, because we are of opinion, that a pre-requisite for deriving useful information from any practical treatise of medicine, is to consider the mode of living of the patients who are the subjects of it, the general state of society in their country, its latitude and climate. That which is fit in one situation, may be totally inapplicable in another, and the person who reads at a distance, should always bear such a caution in mind, lest he found an erroneous practice on rules that are perhaps excellent within their proper sphere. Dr. Trotter represents literary men as subject to the terrible nervous affections of dyspepsia, hypochondriasis, and melancholia. His observations under this head are found too often verified in Europe. Men of genius are usually endued with a large share of sensibility of the nervous system, and those persons often endure the most poignant sufferings under the pressure of a precarious subsistence. But such want is perhaps in no instance to be met with here: neither have we in America, a class of men who get their bread by authorship. Literature alone is not a profession among us, and those of our citizens who recur most to books, such as physicians, lawyers, and the clergy, find in the practice of their professions, the exercise that maintains health, and the emolument that cheers their spirits.

The author seeks the remote causes of nervous diseases principally in crowded cities, where moral evils accumulate most, and animal life is deprived of that pure air so necessary to its maintenance in full health. Some of our cities are already full large, and in proportion as they augment, so will nervous distempers, and others more rapidly fatal. Narrow lanes, high buildings, filthy kennels, small apartments, manufacturing establishments, cellars under ground, consumption of fuel, and a large population, are evils from which the United States are no longer exempt. An inhabitant of the city is not less easily distinguished here than in Europe, by his complexion and countenance. It is the known effect of the oxygenous principle of the atmosphere, when duly inhaled from a current breeze, not only to saturate the blood with floridity, but to exhilirate the spirits in a high degree; invigorate and redden the muscular fibre; bestow energy on every nerve, and call forth at once mental and corporeal strength.

Dr. Trotter, in speaking of his own country, observes that,

> " One of the great misfortunes which some species of manufacture have brought on this country, is the employment of children, before the body has attained its full growth. During infancy and childhood, and till the human frame has acquired its full stature, it would appear, that more pure air is required for the purposes of health than at a remoter period of life. The heart of the child much surpasses that of the adult in the frequency of its pulsations; and every movement of the body is more constant and quicker, so that it seems to be never at rest, but during sleep. Hence the greater expenditure of vital air. The child who is early employed in a woollen or cotton manufactory, is to the village plough-boy what the town lady is to the country girl. They are both examples of nature's precepts being disobeyed: for to be stewed in foul air, is the same in effect wherever it happens.
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> " A warm unventilated nursery is a nuisance of the first magnitude. Nurses and servants have too often the sole superintendance of these apartments: fashionable mothers at least, as some may be justly called, seldom visit nurseries. Bad health, particularly among the delicate children, is too often brought on by a close sleeping place. It is only when the weather is extremely cold that a fire should be permitted in those apartments; for during the sleeping state, the body is not sensible of the deficient purity of air; and it may be substracted altogether, without any struggle being observed, and without the body being waked. It is thus infants are so easily suffocated or overlaid: as the air becomes more impure the stupor increases, till respiration stops; and the child is always found in the sleeping posture.* Sleep is not only the image of death, but it seems a step towards it.

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* In 1801, while I resided at Plymouth Dock, a house adjoining took fire, in an

"The modern practice of confining children to sleep in a crib, till they are three or four years of age, is highly commendable; it has thus all the advantages of the breast to put it to sleep, and is in no danger of being overlaid. The size of the bed afterwards should still be proportioned to the child; and they should never be permitted to lie with a grown person. The convulsions of infants, which are purely a nervous affection, we know to be often brought on by want of ventilation; and we naturally expose them at a window for recovery, as we do with a lady fainting in a crowded room.

"A few years ago, the numerous deaths which happened in a foundling hospital in Dublin, made a great noise in this country. On inquiry, it was found that foul air had the chief share in this uncommon mortality. Some hospitals in Paris, about the same time, were scenes of the like destruction to infant life. Now, children who survive these nurseries and hospitals, or dungeons as they may be called, are so stinted in their health, that they are unfit for every occupation that requires strength; and should they have families, hereditary debility and deformity are the lot of the offspring. Of such vast importance is pure atmospheric air to the growth and health of the human body."

We think it right to give a place here to the author's observations on the abuse of food, because in no country of the world does the mass of population possess so great an abundance of good food as in these United States. The passage will serve at the same time, as a fair specimen of the author's manner.

"3. *Food.*—Luxurious living, joined to other causes, has a manifest tendency to induce nervous and bilious derangement. When a large quantity of rich food is consumed, heightened to the palate by all the arts of cookery, more than nature demands must be taken down. A variety of dishes also invites to inordinate appetite and gluttony. The organs of digestion are thus oppressed and weakened by the surplus; the liver in particular

upper apartment of which, that had no chimney, a poor woman with eight children perished. They were all found in bed, in the sleeping posture; and had been suffocated, from the flame surrounding the room, and exhausting the air, for no part of them was scorched. During the alarm no person thought of this poor family, otherwise the whole of them might have been easily rescued, for only a portion of the premises was consumed. It is probable that none of them waked, as their cries would have been heard, the house being only of two low stories and this garret room.

is enlarged in its volume: the blood is charged with a richer quality, and every vessel and gland stimulated to excess. The nervous system is also preternaturally excited, and grows highly susceptible of stimulus, and easily affected in mind by pleasurable sensation. If a life of indolence is led under luxurient diet, joined to the liberal use of fermented liquors, together they form that habit which constitutes the *voluptuary*. It is characterised by a florid complexion, sparkling eyes, an unmeaning simper, softness of flesh, and fulness of blood, rather than obesity. The mind is light, indolent, and indifferent to every thing but pleasure. A person of this description is perpetually in pursuit of some selfish indulgence; courts the acquaintance of all who keep good tables; flatters the weak part of the sisterhood, but shuns the society of sensible women; and calls philosophers insipid companions, and disturbers of a party. Such a man is remarkable for a great flow of spirits at the convivial board: but his round of delights is not lasting. He that laughs and drinks with every one, grows sad in time; and early decrepitude, and nervous debility are his certain portion. As his youth was remarkable for good health and levity of disposition, so his age is tortured by low spirits and paralitick weakness.

" There are some persons of the nervous temperament that are much addicted to high seasoned food, which I call luxurious living. This custom is often first acquired by indulging in the use of hot articles, with a view to obviate flatulence, and other dyspeptick feelings; but insensibly gains ground, and by repetition it is confirmed into habit; and the excitability or vital power of the stomach is speedily exhausted and worn out. Such persons commonly die of apoplexy, after a voracious repast, or linger a while under incurable palsies of the limbs and other organs, and idiotism.

" Where persons, after a course of high living, are compelled to feed on a less generous diet, we often meet with painful stomach complaints, and low spirits. In these cases, the organs of digestion seem to be so much weakened by the previous excess, that poor fare is unable to stimulate them to healthful action. Under such circumstances men are apt to seek relief from spirituous liquors, which quickly terminate existence, by aggravating all their complaints.

" The use of *tea* in this country, as an article of diet, comes under this head. The consumption of the Chinese plant is enormous throughout the united kingdoms: it is a beverage well suited to the taste of an indolent and voluptuous age. To the glutton it affords a grateful diluent after a voracious dinner; and from being drunk warm, it gives a soothing stimulus to the stomach of the drunkard. But however agreeable may be its immediate flavour, the ultimate effects are debility and nervous diseases. There may be conditions of health, indeed, where tea can do no harm, such as in the strong and athletick; but it is particularly hurtful to the female constitution; to all persons who possess the hereditary predisposition to dyspepsia, and all the diseases with which it is associated; to gout, and to those who are naturally weak nerved. If this observation is just, we may account for many taking it with impunity, and holding out the deleterious qualities of tea as a medical chimera, conjured up to magnify the importance of physick, and to frighten old women. Fine tea, where the narcotic quality seems to be concentrated, when taken in a strong infusion, by persons not accustomed to it, excites nausea and vomiting, tremors, cold sweats, vertigo, dimness of sight, and confusion of thought. In its more diluted state, sweetened with sugar, and softened by the judicious mixture of bland cream, it is grateful to the stomach, gives a soothing sensation, as if it lulled pain, exhilirates the spirits, produces wakefulness, relieves fatigue; and from being taken down warm, promotes perspiration, and acts powerfully by the kidneys.

" But I have known a number of men and women subject to nervous complaints, who could not use tea in any form without feeling a sudden increase of all their unpleasant symptoms, particularly acidity of stomach, vertigo, and weakness of sight. Though fond of tea myself, I have sometimes been obliged to leave it off, by suspecting that it added to my natural shortness of vision.

" As the use of this article in diet extends among the lower orders of the community and the labouring poor, it must do the more harm. A man or woman who has to go through much toil or hardship, has need of substantial nourishment; but that is not to be obtained from an infusion of tea. And if the humble returns of their industry are expended on this leaf, what remains for the purchase of food better adapted to labour? In this case tea comes to be hurtful, not only from its own narcotic quality, but that quality will act with double force in a body weakened from other causes. This certainly is one great reason for the increased and increasing proportion of nervous, bilious, spasmodic, and stomach complaints, &c. appearing among the lower ranks in life. This fact has long been confirmed to me in different countries, and among persons varying much in their employments. I have lately met with many severe and obstinate cases among poor tradesmen and labourers, where it was plain they originated from this cause. I also think that the use of tea often paves the way to habitual dram-drinking among this class of society, more than among the better orders.

"It is worthy of remark, that the finer the tea, it contains more of the pernicious quality; hence the greens which are gathered in the bud, or before the leaves fully expand, have with that colour the strongest flavour. To sip frequently of green tea, produces wakefulness and gaiety of spirits; hence some literary men who protract their studies to a late hour, use strong tea, like the late Dr. Johnson, to keep them awake. It also possesses an emetick quality, on which account it can seldom be taken in the quantity to induce sleep. Dr. Cullen, of Edinburgh, who was subject to gout, said, he never could take it without feeling some arthritick affection of his stomach; yet many dyspeptick people find it highly agreeable, while others feel immediate bad effects from indulging in it.

"The nervous ailments of female constitutions, which are often induced and aggravated by tea-drinking, in advanced age, are apt to terminate in palsy. And from a concomitant torpor of the absorbent system of vessels, they also very frequently terminate in general dropsy. *Coffee* possesses the narcotick principle, but in a lesser degree than tea; the same diseases follow its use, though its hurtful quality is perhaps diminished by roasting. How far the hot water taken with tea and coffee, increases their deleterious effects on the stomach and nerves, may depend on natural temperament. Some persons are in the habit of drinking them very warm, while others prefer them at a lower temperature. The best degree I should think about blood-heat.

"A number of *aromatic substances*, which include all the spices and peppers, as well as some other articles, are in common use as *condiments* of diet. It may be part of natural instinct, that food, to be nourishing and agreeable, should be sapid. Yet the infant at the breast seeks no addition to his milky stream; and it is doubtful to me if any of the warm ingredients are useful. The tropical savage uses pepper with his vegetable meal, in great quantity. Now this kind of diet is watery and insipid. Without something savoury he has no pleasure in eating it; and the stimulus of seasoning becomes necessary.

But the European who imitates this custom, with his turtle and beef soup, while he is a resident in the East or West-Indies, errs egregiously from the indications of nature. All dyspeptick persons are fond of hot articles. When I hear them call for Cayenne pepper at table, it is not difficult to find out their modes of living. It is a key to the temperament, and facilitates the physician's inquiries. But plain fare, such as all dyspepticks ought to feed on, needs no aid of this kind: and I differ widely from those medical authorities who inculcate the practice; the whole of them manifestly weaken the stomach. All the spices owe their warm pungency to a volatile oil: but as far as I have correctly watched their effect, instead of promoting digestion, they evidently disturb it. They float on the surface of the food, and irritate the cardia or upper orifice of the stomach, which has the immediate effect of inverting the motion of the muscular fibres, and then the flatulence appears by rising upwards, and producing eructation. This belching is said to be salutary, though proved to be a symptom of disease. If these articles are ever admissible at all, it must be in advanced life, when the vigour of the stomach and intestinal canal grow naturally torpid and slow. Of themselves they yield nothing of the nourishing principle. Soy, catchup, and all the warm sauces, increasing daily in number, have the same pernicious effects. But when we hear of a dozen of large bacon hams being melted down into a few pint bottles of condiments, that pamper and poison health, while they destroy what would have been wholesome food for numbers, it is a moral evil of great magnitude, and ought to be prevented.

" It is worthy of remark, that condiments of every kind, from custom, become very desirable, till at last no food is relished without them. But it is rather the palate than the stomach to which they are grateful. Some of the peppers are caustick, and cannot fail of injuring, if not corroding the coats of the first passages. Even mustard, which owes its warmth to a fixed thick oil, though one of the mildest of the condiment kind, is capable, in the form of a sinapism, of vesicating the sole of the foot, over which is spread the thickest epidermis on the whole surface of the body. Must not such a substance be hurtful to the delicate stomach? To mustard and pepper I have never accustomed myself, from infancy upwards; and I remain a proof of the truth of my own doctrine, few persons being more exempt from dyspepsia. All these articles ought therefore, to be denied to children, which will be one grand step to make them dislike every hot ingredient in diet, when they grow up. Should brandy, or those waters which are impregnated with the *kernel flavour*, be mixed with sauces of any kind, it is evident they must be noxious. They are dangerous, as being narcoticks the most enticing of all articles applied to the taste; and children that are early accustomed to such poisons, will run great hazard of ending their career as sots and dram drinkers."

We have often had occasion to lament, that the unquestionably good effects of mercury in a variety of cases has brought it now to be the last resort of every pretender who is at a loss for an indication, and that in this way presumption and empiricism are every day productive of the greatest mischief. It will appear from the following extract, that this abuse infests English practice no less than our own.

"An article of *materia medica* possessing such activity in some conditions of the living system, may be capable of producing great and salutary changes. But in effecting these, certain premises and indications are to be attended to, to justify the exhibition: and the physician who does not give himself the trouble to consider these circumstances, may perform the part of an executioner. Mercury, being particularly hurtful where the stomach and bowels are very irritable, is forbid in gout and in the nervous. In those persons, frictions of the ointment lessen the danger. Hence it is improper in women and children; in those subject to epilepsy, or other convulsions; and in every one, who may be liable to spasmodic complaints of any form. In states of torpor and strength, it can be given with more safety: it is supposed to be the best remedy in glandular obstruction, if the attendant debility does not contra-indicate its use. But it is too often to be observed, that those who make most free with the exhibition of mercury, are the least qualified to guard against its bad effects in improper cases.

"I resided a few years ago in a naval sea-port, where I was often consulted for stomach and nervous complaints, by ladies, the wives and relatives of officers, and my other friends in the public service. Many of these females had been residents in warm climates. The great resemblance which the cases had to one another, excited no small degree of surprize at first, till I learned they had been all subjected to a similar mode of treatment. In short, they had been patients of a physician of extensive practice in the neighbourhood, who prescribed mercury for every disease. Most of these women, among whom were some of great mental and personal accomplishments, had been kept for months or weeks under the influence of the medicine; with their throats, mouths, and tongues so swoln and excoriated, and the teeth so loose, as to confine them to a diet of slops. Those who had no stomach complaints when they began this treatment, found them come on after it; and such as had been nervous and dyspeptick before, felt all their ailments aggravated. This physician, not satisfied with the milder preparations, made a common remedy of hydrar. mur.

"A practice not unlike to what I have just related, made some noise in our navy during the late war. A physician on a foreign station took it into his head that all modern diseases originated from Syphilis. It was in vain that officers and seamen pleaded innocence: if they were unconscious of receiving infection themselves, they were told that it must come from their parents, sometimes to many generations back. Few men could answer for their purity on these occasions: a mercurial course was therefore directed; not for days or weeks, but for years; for there was no safety but in continuing it at intervals through life. A medicine of the kind, employed on such terms, could not fail to do harm: some of the most unfortunate stomach affections which ever came under my care, owed their cause to this treatment; a few valuable men were brought to the grave by it, in the circle of my own acquaintance, where I suspected a schirrous pylorus.

"Mercury, exhibited in quantity, either by the mouth or by friction, very quickly excites an artificial fever, ushered in by chills, shivering, and a hot stage. The pulse becomes frequent; at first full, but latterly weaker and quicker: want of appetite, nausea, and often vomiting, commence; restlessness and night sweats supervene; the body bears the cold air ill; head-ache; fœtid breath; the bowels are commonly oppressed with flatulence, severe twitches, purging, tenesmus, and even bloody stools; to these symptoms, debility and emaciation succeed; the tongue, mouth and throat inflame, swell, grow painful, and ulcerate, and the saliva is poured forth in a continual stream. During the effects of mercury on the body, the mind becomes irritable and unequal, but generally low spirited; and after some time, exhibits all those feelings usually called nervous, inducing the hysterick affection, convulsions, and frequently the epileptick paroxysm itself. It is therefore plain, that in the nervous temperament, all the phenomena now described, must appear with greater violence, as is observed in children and women, and in all who have weak bowels.

" The constitutions of children, in point of debility and irritability, approach to the female habit of body: the nervous power is liable to irregular motions; easily affected by stimuli, and prone to convulsions. Hence mercury is apt to have dangerous effects upon them. With some medical people, as well as parents, this medicine is much employed as a vermifuge: it has the advantage of being easily disguised, and if joined with a purgative, acts very briskly, and commonly brings away many of those vermin. But worms being only the consequence of a weak condition of bowels, mercury, instead of correcting the cause, tends still more to debilitate the first passages when continued long, and lays the foundation for much ill health. I shall touch on this part further, in the treatment, as being intimately connected with my inquiry. In the mean time, I cannot sufficiently reprobate the conduct of some persons in high life, who permit their names to be published in the handbills and advertisements of an illiterate quack, whose worm medicine is known to be mercury, and liable to be given in very unequal doses, from the nature of the composition.

" About twenty years ago, two children of a friend of mine took calomel from the family apothecary, for worms. The exact dose in which it was given I do not recollect; but the motions which it produced were innumerable. The family, during this process, were all at once alarmed, by the singular noise which the children made in their throats, with an unusual change of countenance and cast of the eyes; which were quickly followed by violent agitations and contortions of body, and, lastly, convulsions. In a few days these odd complaints disappeared, as the effects of the mercury abated: but the apothecary thought proper to repeat the calomel, obstinately contending that the fits were mere affectation, and not to be imputed to the medicine. The same effects however appeared after the second exhibition; the evacuations were not less profuse and debilitating, and the convulsions more frequent and severe. The moment one child was seized, the other was affected in like manner: so certain was this kind of nervous sympathy, that there was a necessity for separating them, that they might not see or hear one another, and the fits went gradually off. One of them, a young lady, has been long a martyr to the most painful nervous complaints; and I cannot help thinking that much of this indisposition was brought on by this vermifuge process.

" This mineral, after long use, besides exhausting the nervous energy, is known to affect the bones, and render them friable, just as they are found in rickets and old age. A poison so subtle and active, thus consumes the vigour of body, and brings on premature senility. Those hideous effects are most probably produced by the mercurial oxide first depraving the digestive powers, preventing assimilation of the chyle, and vitiating sanguification. On the whole, while mercury is capable of great good in the hands of the sagacious physician, when indiscriminately used by the ignorant of the profession, it must do incalculable mischief."

Those articles called bitters, require great caution in the exhibition, as possessing the power of exhausting the excitability of the stomach by long use.

"They impede fermentation in the *primæ viæ*, and correct acidity; these they effect chemically. When long continued, as in the history of the Portland powder for gout, they are said to produce palsy and dropsy, and every species of nervous debility. There must, therefore, be a period when the good effects cease, and which careful observers will mark. If *bark* and *bitters* are medicated by alcohol, in the form of tincture, it is obvious the hurtful effects, after a certain time, must be inevitable.

"The *aromatic substances*, among which may be reckoned all the spices and volatile oils used in either diet or medicine, by repetition become hurtful to the stomach, whether as condiments, with food, or to cover the disagreeable flavour of some articles. These substances have, therefore, a limited time to do good, that their use may not become habitual. There is always something wrong in those stomachs which hanker after hot ingredients: when these persons are served with watery preparations of medicine, they complain that they are too cold, and disagree with them; but the moment a spirituous tincture is substituted, there is little danger of its being refused. It is the duty of every honest physician to resist those cravings of his patient, and to endeavour all in his power to correct hurtful propensities."

For the same reason that we have extracted some passages applicable to the state of society in our own country, we shall place here the article on narcotics.

"NARCOTICKS.—These include ardent spirits, opium, and all those articles commonly called anodynes, hypnoticks, paregoricks, &c. such as lactuca, bang, belladonna, hyosciamus, laurus cerasus, cicuta, &c. Of the effects of ardent spirits, in producing stomach and nervous complaints, I have treated largely in my Essay on Drunkenness, to which I refer the reader.* All the articles now enumerated, act very much alike on the human body. In small quantities, they induce vigor, activity, and strength, and an increase of muscular power throughout the frame; at the same time are felt serenity, pleasure, and courage of mind. In larger doses they bring on sleep, stupor, and delirium; and when carried to the utmost quantity, insensibility, apoplexy, and death. It is of little moment in this inquiry, whether narcoticks ought to be considered as directly or indirectly sedative. They are forbid in all inflammatory diseases, where they certainly do harm in the first stage: they generally occasion constipation, but hyosciamus has a laxative quality. When long continued, they are known to weaken the nervous system in a surprising degree; disposing to amentia, epilepsy, palsy, tremours, convulsions, melancholy, madness, &c. No substances in nature more certainly injure the powers of digestion, and bring on all the severe symptoms of nervous infirmity. This effect of narcoticks, is most likely primarily derived from the nerves of the stomach, with which they come first in contact, and thence extended to the other viscera. Where there is a predisposition, or hereditary nervous temperament, or gout, the permanent use of them is still more quickly hurtful: in such states of the body, they ought, like vinous spirits, never to be prescribed but from necessity.

"*Opium*, the noblest attribute of medicine, so calculated by its powers to sweeten life, and suspend pain, when all earthly comforts avail nothing, has its limits in doing good. It is a misfortune when it comes to be dispensed by injudicious hands; for it is often prescribed by the most ignorant, in diseases where it is forbid. The bodily complaints of the human race, when enervated by luxury and refinement, seem to produce more acute pain, at least the temperate man is observed to bear sickness with more patience and resignation, than those accustomed to indulgence. The spirits are apt to flag, as if the mind had no resting place. Opium alone gives relief, though it must feed the disease. Such persons seem to compound with their physician for sound nights, and days of ease; and if he does not comply, he must be changed. Hard is the task imposed on the medical attendant; he must obey, or starve. The *night draught* thus becomes familiar in the family; the servant goes to the apothecary for it with as little ceremony as he buys kitchen salt. He sees the shop-boy count the drops into the phial, and when he gets home, narrates the composition of the placebo to the cook and the nurse-maid. Not a domestic in the house but soon learns what a fine thing laudanum is;—and master swears he can get no rest without it.

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* "Second edition. London, 1805."

"If such things did not exist, how comes it that seven young women in this neighbourhood, within the last three years, should have known that a large dose of this tincture will kill. Two of the number effected their purpose. I was called to visit another in conjunction with Mr. Elliot: she had swallowed twelve drams of laudanum; but I arrived before it had been twenty minutes in her stomach, and by plentiful dilution with vinegar and water, followed by an active emetic, the bad effects were prevented. I remained in the house till I was certain that every drop of the opium was discharged, and she did well. It appeared that these poor girls had become the dupes of designing men, and called for death to end their sufferings. Thus the dose of opium concludes what was begun in the circulating library. A little more secrecy and discretion are certainly wanting in the general use of this dangerous narcotic.

"But there is reason to believe, that even medical men themselves, have of late entered too easily into the indiscriminate use of opium. He must be a short sighted physician that does not calculate upon the ultimate effects of his prescription: it is a weak excuse for getting quit of the importunities of a patient, by complying with an improper request, that may afford temporary ease, at the expense of permanent health. In the nervous temperament it is particularly hurtful. I am acquainted with numbers of ladies that feel such horror at taking it, as nothing can equal; and in every illness they may labour under, constantly warn the medical visitor about giving it, as no disguise can make it agreeable to them. But, when opium happens to be soothing to weak nerved people, from their quick sensations, it is apt to be the more craved for, and converted into habit. The languor and dejection which follow its operation pave the way for the repetition of the dose, till general debility succeeds. In such constitutions, the exhibition of opium ought never to take place on slight occasions. Midwives, nurses, and other persons out of the medical profession, who dispense laudanum at random, ought to be solemnly warned against it

"The *opium-eaters* in Turkey, are the most pitiable objects in society. Their squalid looks and emaciated bodies, after a long course of this narcotick, evince how fatal its use is to the digestive organs, and how it prevents the due assimilation of nourishment. On dissection, the same effects appear in the stomach, liver, and bowels, which follow a long course of ardent spirits. Like the dram drinker also, when deprived of his wonted cordial, they are languid, faintish, low-spirited, nervous, and feeble. Their limbs totter under them; their heads and hands tremble, and the very wind is in danger of oversetting them. Their faculties seem exhausted, the memory fails, and the only remnant of intellect which they possess, appears in the frightful accounts which they give of their horrors, and the hypochondriacal glooms with which they are infested. No form of disease exhibits existence under a more deplorable shape than the *opium eater* and *dram drinker*. There is great reason for suspicion, that this drug is daily getting more into use as a cordial, and privately consumed by numbers of persons. It is well adapted from its preparation in tincture, to be carried about, and drank at pleasure. Its exhilirating quality creates a momentary heaven for minds who find nothing but guilt and despair in their own reflections: hence it has grown so general in fashionable circles. I am also of opinion that many sudden deaths are, in the present times, occasioned by this drug, without exciting the least suspicion of its being taken. Those who accustom themselves to the narcotick article called *bang*, in the East-Indies and in the Turkish dominions, are said to perish under diseases similar to those of the opium eater.

" *Tobacco* is another narcotick in common use. Persons who are in the habit of chewing, snuffing, or smoking this Indian leaf, are not aware, that a few grains of it, taken into the stomach, cause sudden death. Nay, the smoke of it injected into the rectum, has frequently proved fatal. It powerfully acts on the nervous system, destroys the sensibility of the stomach; and it is observed that those who devour it in great quantity, die of apoplexy, palsy, and dropsy."

In his sixth chapter, the author delivers his general doctrine of nervous diseases. He considers them all as flowing from deranged sensation, and to be found in most abundance where luxurious habits have displaced simplicity of living. He has known them less among Quakers than any other description of people, and this is some proof of the sobriety and regular habits that distinguish *the Friends*.

In the seventh and last chapter, which Dr. Trotter entitles "prevention and treatment," he observes, that,

"It has been unfortunate for the medical profession, as well as patients themselves, that persons labouring under nervous disorders have too much expected from the prescription of the physician, and the shop of the apothecary, what is only to be obtained from their own caution and circumspection. We thus find most of them ready, and greedy to swallow every medicine that is recommended; but stubborn and untractable in all that relates to breaking in upon established habits and customs; whether of luxurious living, depraved appetites, indolence of body or mind, or vicious indulgence of any kind inconsistent with health. Many of these habits, it is true, are so far interwoven with the constitution, as to make some changes almost impracticable: but as indisposition is so frequently brought on, or aggravated, by the improper conduct of the patients themselves, the physician cannot be too much on his guard, in demonstrating to them all that belongs to their own government and demeanor. The medical adviser, therefore, who observes the most disinterestedness towards his friends, will often be the first man to be dismissed; while the selfish dissembler, however ignorant, will become a favourite, and engross the emolument. On such an occasion, the virtuous mind of a liberal physician will know where to look for approbation.

"This branch of medical practice has commonly been reckoned one of the most lucrative; for the subjects of it are generally found among the affluent: they are also seldom without some complaint that requires assistance; and they measure their comforts too often by the quantity of medicine that is served up. Nervous people are, moreover, endued with acute feelings; liable to act from the first impression and impulse, and easily deceived by the designing and interested. And should they fall into the hands of a gossiping physician, or a wheedling apothecary, these personages become a kind of appendage to their establishment, if not fixtures in their houses. Being singular in the selection of friends, they seldom mix in company; sedentary from habit, they go little abroad; their amusements and recreations are thus limited, and such as possess the talent of bringing news, and telling a story, are at all times welcome guests. But as the tale of their own complaints engrosses so much of their conversation, a medical gossip, before all others, is the most acceptable. Nevertheless, let the nervous and valetudinary beware how they trust their health and their purse in such hands."

"From the experience which I have had in some thousand of these cases, under all the variety in which they usually appear, I freely give it as my opinion, that the only *means of cure* lie in a total abstinence from every species of spirit or fermented liquor; from every thing that bears any analogy to them, such as tea, coffee, opium, and all other narcoticks; and to regulate the diet, clothing, air, exercise and passions as becomes a rational being. The improper use of these articles being the *chief cause* of nervous indisposition, it follows that no recovery can be perfect, till they are *in toto* discontinued.

"Attention to the state of the bowels forms an important part of the prevention: but if due regard is had to air, exercise, and diet, even habitual constipation may be overcome. What relates to its relief otherwise, will be noticed in the proper place; but if the food can be so regulated as to supersede medicine, it is an important point gained.

"The whole of the precepts which have been delivered, as the means of prevention against this singular train of diseases, it will be readily perceived, turn upon a *reversed mode of living*, or changes opposed to former habits; and to bring our patients back to a simple regimen of food, air, exercise, &c. Our plans comprehend no favourite theories, nor any intricate doctrines; they aim at being natural, simple, and easy to be understood."

In the practical part of the work, the author opposes giving tonic medicines in too large doses; bark is undoubtedly thrown in by some practitioners in such doses as to clog, not to strengthen the stomach; but from iron he expects most advantage, and agrees in general with Sydenham, in preferring it in its metallic state. He admits, however, that there are conditions of the system in nervous diseases, where some of its saline preparations are to be preferred.

"In some female habits where there is vast disposition to acidity of stomach, the limatura ferri, and carbonas ferri, will act most powerfully. And there is reason to suppose, that this very acidity of the primæ viæ further oxydates the metal; by that means destroys itself, and leaves the iron in a more active state to strengthen the fibre, or to mix with the blood, and exert its efficacy on the whole circulation. But in cases where there is little or no acidity in the first passages, the muriated tincture of iron, and vitriolated iron, have appeared to me to have manifest advantage. Again, in cases where there is much torpor, or as you may call it, sluggishness of fibre, and coldness of constitution, with acescency, then the ferrum ammonicale, or the tincture made from it, are of superior efficacy. Some of the most fortunate recoveries which ever occurred under my own prescription, were effected by the ferum ammonicale. In these cases, violent pain of the gastrick region, occasional jaundice, flatulent cholick, shifting spasms of the whole abdomen, vast acidity, vertigo and head-ach, &c. were the predominant symptoms. Yet most of these cases were reduced to the last degree of debility when I first saw them. Certainly in stomach and bilious complaints under extreme weakness, very unexpected returns to health sometimes take place by a fortunate change of medicine. It must therefore be a point of nice discrimination in the practice, to suit the preparation of this metal to the exact disposition of the disease, and the idiosyncrasy of constitution.

" When I employ chalybeates of any description, it is usual with me to precede their use, for a few days, with some preparation of bark or bitters; and to continue them for two or three weeks with the ferruginous medicine. After a short interval they may be again renewed, as circumstances require. In this manner the two articles will effect what neither of them can do alone. But I am of opinion, with many other physicians, that ferruginous medicines of all kinds ought to be continued for a length of time, if full benefit is to be derived from them. Even where the good effects are early apparent, they must not be too soon laid aside: from three to six months are often required to insure all the good that may be obtained. All the different forms of iron are apt to make the bowels slow; which, with some patients, is a constant objection to their employment. Even the sagacious Sydenham went so far as to say that laxatives ought not to be given during their use, as weakening their efficacy. It is, however, to be remembered, the diet of Englishmen in his days was not so full and pampering as in the present times. This opinion of Sydenham has, however, had very little weight with me: I not only interpose laxatives during the use of iron, but sometimes order a smart purge at intervals; and so far am I from thinking that it weakens the power of the other medicine, I have always thought it necessary for securing its best efficacy. The office of the intestines must be preserved regular in such patients, if any good is to be done. The kali tartar. is perhaps the best purgative article at this season; and it may have some action on the chalybeate that we cannot well explain. If, however, our regimen of air and exercise is strictly complied with, there will be the less necessity for forcing the bowels with medicine.

"When iron is adapted to the case, the good effects are first observed, in the decline of the dyspeptic feelings: the calls of appetite become more frequent; the food digests with more ease, and acidity and flatulence cease to be painful. At this time the colour of the face improves, and grows florid; and the skin, from being sallow and dry, becomes better coloured, soft, and perspirable; the alvine fæces are commonly tinged during the use of iron. With these changes, strength of body and alacrity of mind soon appear. In delicate females, some weakening sexual symptoms quickly decline.

"The indisputable good effects, which have so often resulted from a trial of *Bath waters*, in many varieties of these diseases, render it almost certain, that these have been produced by the ferruginous principle which they possess; but which is only effectual at the fountain, as it soon precipitates, and cannot be preserved. As we perceive this metal, when artificially managed, to effect surprising changes in the animal economy, it is but fair to allow, that much more may be expected from the fine chemistry of nature. It is in vain to deny this, because the Bath water contains an extremely minute portion of iron, discovered on analysis: the effect must rest on the activity of the ingredient, whether in the state of oxide or not, not on its quantity. The sensible qualities of the water, when drank on the spot, are highly ferruginous; and the stimulant power is so perceptible, that in some habits of body, it is dangerous; inducing temporary fever and vertigo, and in some instances apoplexy. The torpid habit is, therefore, most relieved by them; persons worn down by the diseases and excesses of tropical climates, and those debilitated by luxurious and hard living at home. The regular hours, correct manners, and elegant pleasures of the company at this fashionable place of resort, become the best antidotes in life against licentious drunkenness: no wonder then the enervated debauchee should return healthy, and sometimes reformed, from a course of Bath waters. But when nervous, bilious, and dyspeptick complaints depend, as they often do, on a *gouty diathesis*, Bath waters are a sovereign remedy. The energy which they quickly impart to the chylopoietick organs, is soon extended over the whole frame; and all those anomalous symptoms, usually called *flying gout*, seem to rally to a point, and cease when the affection comes to be fixed in the extremities. I am thus induced to mention these salutary springs, under the method of treatment."

In general, the scope of the practical part of this popular work is to recommend, by precept and example, temperance and exercise to all persons of a nervous temperament, directing the attention of the physician and the patient to a due attention to the affections of the mind, no less than to those of the body. As the work is written in an agreeable manner, devoid of technical expressions, we think it well calculated for attaining its object. It may be especially useful to those who, though not of the profession, are fond of dipping into medical books, and they are very generally persons of the nervous temperament.

A Treatise on the Process employed by Nature in suppressing the Hemorrhage from divided and punctured Arteries; and on the Use of the Ligature; concluding with Observations on secondary Hemorrhage: the whole deduced from an extensive Series of Experiments, and illustrated by fifteen Plates. By J. F. D. Jones, *M. D. Member of the Royal College of Surgeons of London.* 8vo. pp. 237. *London.* R. Philips. 1805.

THIS work, notwithstanding the length of time it has been printed, and the celebrity it has acquired in Great-Britain, is little known in this country. We should have noticed it in our first number, but could not procure a copy sufficiently early, notwithstanding all our endeavours for that purpose; we will now gratify our readers with an abstract of the important information it contains.

The author states, in the advertisement prefixed to the work, that though small, it is the fruit of much labour and expense. Of this we have no doubt, as he has completely verified by experiment every fact which he states, and every conclusion which he draws. Our limits preclude our laying his experiments before our readers; we can state the results only, referring those who wish to peruse the former to the work itself.

In the first chapter the author treats "of the process which nature employs for suppressing the hemorrhage from divided arteries." In the first place he gives the following sketch of the various hypotheses which have prevailed upon this subject.

> "Mr. Petit conceived that hemorrhage, from a divided artery, is stopped by the formation of a coagulum or clot of blood,* which lies partly *within* and partly *without* the vessel. The external portion is formed, he thought, by the last drops of blood which issue from it; the internal portion, by the blood contained just within its divided extremity; the former he called the 'Couvercle;' the latter, the 'Bouchon.' The clot, he said, afterwards adheres to the internal coat of the vessel, to its orifice, and to the surrounding parts. To this he added, that when hemorrhage is stopped by the application of a ligature, a clot is formed above the ligature; and the coagula, under these different circumstances, differ only in figure. This view of the subject led him to recommend compression to support the clot, that it may not be pushed away by the impulse of the blood."
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> "In the year 1736 Mr. Morand published a Memoir, in which he added many very interesting remarks to those which had been made by Mr. Petit. He admitted that the formation of a coagulum had some effect in stopping hemorrhage; but contended that the changes which the artery undergoes also contributes to that effect. These changes he described to be a sort of corrugation or plaiting of the circular fibres of the artery, by which its canal is diminished; and a shortening and consequent thickening of its longitudinal fibres, so as nearly to fill it up: this latter effect he conceived to be produced, when an artery is completely divided, and that it constitutes the principal means by which, in such cases, the hemorrhage is stopped. He added, that the corrugation, or puckering of an artery may take place, so as to obliterate its cavity, provided pressure be made round it; as by a ligature."
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> "Mr. Sharp, nearly at the same period, supported the same doctrine, as may be found in the second edition of his Opera-

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* "Caillot de Sang."

tions of Surgery, published in 1739. It is more concisely and better expressed than Morand's; thus: 'The blood-vessels, immediately upon their division, bleed freely, and continue bleeding till they are either stopped by art, or at length contracting and withdrawing themselves into the wound, their extremities are shut up by the coagulated blood.'—This doctrine Mr. Sharp gives incidentally; leaving it unsupported by experiment or observation; so that it does not appear whether it is his own, or borrowed from another. It is not comprehensive enough; but, as far as it goes, it accords with the truth.

"The next theory which I am to notice is Mr. Pouteau's. It is prefaced with many very judicious observations, peculiarly applicable to the subject; and from which one might have expected that he had completely exhausted it, and had left nothing for future inquirers to discover or confirm. 'Le desir si louable de découvrir le vrai, doit être dirigé dans sa marche par un grand nombre d'experiences: . . . . pour se persuader qu'on a bien vu, il convient d'avoir regardé long tems et sous differents points de vûe."*

"Mr. Pouteau denied that a coagulum is always to be found after the division of an artery; and when it is, he thought it should be considered only as a feeble and subsidiary means towards the suppression of hemorrhage. The retraction of the artery, he said, has not been demonstrated; and that, at any rate, it is not more effectual than the coagulum. He asserted, that the tumefaction of the cellular membrane, at the circumference of the cut-extremity of the artery, forms the principal impediment to the flow of blood; and the application of a ligature to an artery is useful, by promoting a more immediate and extensive induration of the cellular substance."

"Mr. Gooch, Mr. White, and Mr. Aikin, considered the experiments and observations of Mr. Kirkland quite conclusive in confuting the doctrine of a coagulum, and in establishing the contraction of the arteries as the means which nature employs in the stoppage of hemorrhage from divided arteries. In justice, therefore, to the subject, and to Mr. Kirkland, I shall extract some of the most important of those observations on arteries which he had the opportunity of making.

"Mr. Kirkland, considering that, when the impulse of the blood is abated by the swooning of the patient, it is very common for considerable arteries to be closed by their natural contraction; and convinced by experiment, that the close contraction observed in arteries to which agaric has been applied, is owing to the firm adhesion of the agaric to the artery, by which its mouth is shut, was led to determine the effect of a temporary pressure sufficient to stop the flow of blood from an artery. The conclusions and opinions drawn from his experiments are here gathered, and brought into one point of view, viz.

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* "Melanges de Chirurgie."

"That hemorrhage from a very considerable artery is easily and effectually suppressed by only making a perpendicular pressure upon the end of the vessel for a few minutes.

"That the pulsation, at first, is very plainly seen at its extremity; but after some time it becomes less perceptible.

"That the bleeding is not suppressed by congealed blood, but by the vessel being quite close contracted for near an inch or more from its extremity: that by removing the pressure from time to time, it is easy to discover this contraction taking place by a gradual decrease of the stream of blood.

"That the artery collapses and gradually closes itself up to the nearest lateral branches, as the resistance towards the ligature abates from the blood passing through those vessels. It appeared to Mr. Kirkland even self-evident, that nature always took this step to suppress the hemorrhage from divided arteries, upon reflection, that the same circumstance constantly happens when the umbilical vessels are divided at the birth of the fœtus.

"That the impulse against the end of the artery immediately becomes less, and soon entirely ceases from the blood, upon meeting with resistance, dilating and passing through the nearest lateral branches; and this he thought evident from the gradual decrease, and, at length, the total disappearance of pulsation.

"That the contraction of the artery, and the alteration in the course of the blood is not long in taking place; after which it probably, like the umbilical vessels, shrinks into a perfect cord, and becomes impervious.

"That intercepting the passage of the blood for a while is all that is required from art."

"The last theory which I have to notice, is that lately published by Mr. J. Bell,* who, after freely criticising those which I have already mentioned, confidently asserts, that, 'when hemorrhage stops of its own accord, it is neither from the retraction of an artery, nor the constriction of its fibres, nor the formation of clots, but by the cellular substance which surrounds the artery being injected with blood.' It is to be regretted, that among the plates which Mr. Bell has very judiciously introduced to illustrate the doctrines of Petit and Pouteau, he has not added one to exemplify his own, which, although it appears to be delivered in very decisive terms, yet, in its affirmative part at least, is vague and inconclusive. It was, perhaps, on this account that the author subjoined a copious illustration of it, which I shall also quote, that I may not incur the reproach of having given only a partial and unjust view of his theory.

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* Principles of Surgery.

'The stream of blood gradually lessens, because the artery is emptied, and the resistance to the arterial action taken away; the stimulus being gradually lessened, the artery every moment acts less powerfully; and the blood being no longer solicited or urged on by the arterial contractions, forsakes the open artery, and moves along the neighbouring branches. The surgeon claps the point of his finger upon the mouth of the artery, and holds it there; the outward bleeding is prevented; the blood is extravasated into the cellular substance around the mouth of the artery; the cellular substance is slightly injected with blood; that blood coagulates, and that slight barrier is sufficient to restrain the bleeding of a small artery, till the parts inflame, and the artery is entirely stopped.

'Supposing the artery still larger and more powerful, and that it drives its blood very furiously among the cellular substance, it is not this slight injection of the cellular substance that will restrain the bleeding. Whenever the finger is removed the blood bursts through this slight impediment. The injected cellular substance will not support the artery, unless the cellular substance itself be also supported.

' . . . . . Retraction of the artery has no effect in suppressing hemorrhagy, but as it fills the cellular substance; and this injection of the cellular substance is but a slight obstacle, fit to support only the very smallest arteries. The natural powers which restrain hemorrhagy do but suppress it for a time, and expose the patient to secondary hemorrhage!'

After detailing the experiments from which his conclusions are drawn, the author corroborates his statements by the various facts connected with this subject recorded by different medical writers, from which facts, however, they had drawn very different conclusions. The following then is the manner in which this process really takes place.

" The results of the experiments related in the last section will not allow us to give so concise and simple an account of the process, as has hitherto been done; but they afford us one more satisfactory, because it accords better with the operations of the animal œconomy, in which we are accustomed to observe the most important changes gradually produced by the co-operation of several means, rather than by the sole influence of any one in particular.

" They accordingly show that the blood, the action, and even the structure of arteries, their sheath, and the cellular substance connecting them with it—in short, that all the parts concerned in or affected by hemorrhage, contribute to arrest its fatal progress, by operating, in the case of a divided artery of moderate size, in the following manner.

" An impetuous flow of blood, a sudden and forcible retraction of the artery within its sheath, and a slight contraction of its extremity, are the immediate and almost simultaneous effects of its division. The natural impulse, however, with which the blood is driven on, in some measure counteracts the retraction, and resists the contraction of the artery. The blood is effused into the cellular substance between the artery and its sheath, and passing through that canal of the sheath which had been formed by the retraction of the artery, flows freely externally, or is extravasated into the surrounding cellular membrane, in proportion to the open or confined state of the external wound. The retracting artery leaves the internal surface of the sheath uneven by lacerating or stretching the cellular fibres that connected them. These fibres entangle the blood as it flows, and thus the foundation is laid for the formation of a coagulum at the mouth of the artery, and which appears to be completed by the blood, as it passes through this canal of the sheath, gradually adhering and coagulating around its internal surface, till it completely fills it up from the circumference to the centre.

" A certain degree of obstruction to the hemorrhage, which results from the effusion of blood into the surrounding cellular membrane, and between the artery and its sheath, but particularly the diminished force and velocity of the circulation, occasioned by the hemorrhage, and the speedy coagulation of the blood, which is a well known consequence of such diminished action of the vascular system, most essentially contribute to the accomplishment of this important and desirable effect.

" A coagulum then, formed at the mouth of the artery, and within its sheath, and which I have distinguished in the experiments by the name of the external coagulum, presents the first complete barrier to the effusion of blood. This coagulum, viewed externally, appears like a continuation of the artery, but on cutting open the artery its termination can be distinctly seen, with the coagulum completely shutting up its mouth, and enclosed in its sheath.

"The mouth of the artery being no longer pervious, nor a collateral branch very near it, the blood just within it is at rest, coagulates, and forms, in general, a slender conical coagulum, which neither fills up the canal of the artery, nor adheres to its sides, except by a small portion of the circumference of its base, which lies near the extremity of the vessel. This coagulum is distinct from the former, and I have called it the internal coagulum.

"In the mean time the cut extremity of the artery inflames, and the vasa vasorum pour out lymph, which is prevented from escaping by the external coagulum. This lymph fills up the extremity of the artery, is situated between the internal and external coagula of blood, is somewhat intermingled with them, or adheres to them, and is firmly united all round to the internal coat of the artery.

The permanent suppression of the hemorrhage chiefly depends on this coagulum of lymph; but while it is forming within, the extremity of the artery is farther secured by a gradual contraction which it undergoes, and by an effusion of lymph between its tunics, and into the cellular membrane surrounding it; in consequence of which these parts become thickened, and so completely incorporated with each other, that it is impossible to distinguish one from the other; thus, not only is the canal of the artery obliterated, but its extremity also is completely effaced, and blended with the surrounding parts.

"From this view of the subject we can no longer consider the suppression of hemorrhage as a simple or mere mechanical effect, but as a process performed by the concurrent and successive operations of many causes: these may briefly be stated to consist in the retraction and contraction of the artery; the formation of a coagulum at its mouth; the inflammation and consolidation of its extremity by an effusion of coagulating lymph within its canal, between its tunics, and in the cellular substance surrounding it."

In his fourth chapter the author gives the following account of the process of adhesion, and the changes which an artery finally undergoes in consequence of the application of the ligature.

" The first effects of the ligature upon an artery are, a complete division of its internal and middle coats, an apposition of its wounded surfaces, and an obstruction to the circulation of the blood through its canal. The artery does not become unusually distended in consequence of this obstruction, principally because the collateral branches afford a passage to the blood; and partly in consequence of the effect which the obstruction to the arterial canal, and the pressure made by the ligature, have in depriving that portion of the artery of the property of accommodating itself to the quantity of blood determined to it: a property which it has frequently been observed arteries are endued with, to a certain extent, when entire and free from pressure, and which is beautifully illustrated by the almost immediate enlargement which the collateral branches have been observed to undergo, when a trunk has been tied. From these circumstances it appears, that the enlargement of the portion of the artery between the first collateral branch and its extremity is prevented; but it is obvious that there must be a small quantity of blood just within the extremity of the artery, and which is more or less completely at rest; it therefore coagulates, but does not appear in every instance to form at once a coagulum capable of filling up the canal of the artery; for, as may be observed in many of the experiments, several hours after the artery had been tied, there was only a slender coagulum formed in its extremity. I am therefore disposed to think, that although the artery cannot accommodate itself to the blood determined to it, yet it undergoes such a degree of contraction, as occasions too much motion in the blood which it contains, to admit at once of its complete coagulation. It is a fact, that in most cases only a slender coagulum is formed at first, which gradually becomes larger by successive coagulations of the blood; and for the same reason it is that the coagulum is always at first of a tapering form, with its base at the extremity of the artery. But the formation of this coagulum is of little consequence; for soon after the application of the ligature, the extremity of the artery begins to inflame; and the wounded internal surface of its canal, being kept in close contact by the ligature, adheres and converts this portion of the artery into an impervious, and, at first, slightly conical sac. It seems to be entirely owing to the effusion of lymph by which this adhesion is effected, that the coagulum of blood, formed within the artery, is sometimes found adhering by a small part of its *base* to the extremity of the artery. But whilst the adhesion of the internal parietes of the artery is accomplished, a considerable quantity of lymph is effused between its coats, and among the parts surrounding its extremity; so that in a very short time the extremity of the artery is enveloped in lymph, and covered with a layer of it, just as we have seen the punctured artery to be. After a short time the ligature occasions ulceration of the part around which it is immediately applied; and, acting as a tent, a small aperture is formed in the layer of lymph effused over the artery; through this aperture a small quantity of pus is discharged, as long as the ligature remains; and, finally, the ligature itself also escapes, and the little cavity which it has occasioned granulates and fills up, and the external wound heals in the usual manner; leaving a considerable thickening and induration of the cellular membrane, extending a little beyond the extremity of the artery.

" But since a certain degree of ulceration and formation of pus appears to be the necessary effect of the ligature, and this ulceration takes place as near as possible to a newly cicatrized part, and as it is a well known fact, that such parts very readily ulcerate, it is obvious, that every possible means should be employed to prevent the extension of this ulcerative process: hence we should guard against the accumulation of pus about the extremity of the artery, by such pressure as the parts may conveniently bear; by placing the limb in such a position as will allow a ready exit to the pus; and, in some instances, by the application of sponge.

" It is certainly desirable, that the ligature should come away as soon as possible; yet I am convinced, that great care is necessary in endeavouring to expedite this event. We should always remember, that so long as the attachment of the ligature appears to have any degree of firmness, any force exerted on it (as for instance, by pulling it rather strongly) must act more or less on the recently cicatrized extremity of the artery, which is not only contiguous to it, but is still in union with that portion of the artery which detains the ligature; for it is the external coat of the artery which hinders the separation of the ligature; partly, in consequence of its undergoing ulceration slowly, and partly, because the ulceration, which takes place in the first portion of the artery, by rendering the ligature loose, diminishes very much its power of promoting ulceration through the rest of the artery. But so long as any portion of this external coat remains entire, it is continuous with the external coat of the newly cicatrized portion of the artery, and therefore any force exerted on the former may be extended to the latter:

hence it appears most prudent, that so long as our attempts to draw away the ligature meet with much resistance, they should only be exerted in such a gentle and gradual manner, as may promote ulceration of the part by which the ligature is confined, without endangering laceration of the recently united parts.

"The permanent changes which take place in an artery, and in the circulation through a limb, in consequence of the application of the ligature, are precisely the same as those which happen from the division of an artery, and which I have described in Chapter I. Section III. The portion of the arterial trunk which has been tied, undergoes a gradual contraction and obliteration to the first collateral branches, and finally dwindles to a mere fibre.

"The collateral branches are unusually distended and excited to stronger action from the moment that a complete obstruction is formed in the trunk, and, consequently, the commencement of their enlargement may be referred to that period. Their increase of size seems to be proportioned to the exigencies of the particular case; thus, if the limb has been amputated, it does not appear to be finally very considerable; but if the limb remain entire, and only the natural course of the circulation be obstructed through the main arterial trunk, their enlargement is much more conspicuous, and is particularly observable in the small inosculating ramifications of the collateral branches, by which the circulation appears to be carried on, after a certain time, as vigorously in the limb, the principal artery of which has been obstructed, as in that which has preserved its natural circulation.

"The effects of tying an artery properly appear then to be the following:

"1°. To cut through the internal and middle coats of the artery; and to bring the wounded surfaces into perfect apposition.

"2°. To occasion a determination of blood on the collateral branches.

"3°. To allow of the formation of a coagulum of blood just within the artery, provided a collateral branch is not very near the ligature.

"4°. To excite inflammation on the internal and middle coats of the artery, by having cut them through, and, consequently, to give rise to an effusion of lymph, by which the wounded surfaces are united, and the canal is rendered impervious: to produce a simultaneous inflammation on the corresponding external surface of the artery, by which it becomes very much thickened with effused lymph; and, at the same time, from the exposure and inevitable wounding of the surrounding parts, to occasion inflammation in them, and an effusion of lymph, which covers the artery, and forms the surface of the wound.

"5°. To produce ulceration in the part of the artery around which the ligature is immediately applied, *viz.* its external coat.

"6°. To produce indirectly a complete obliteration, not only of the canal of the artery, but even of the artery itself to the collateral branches on both sides of the part which has been tied.

"7°. To give rise to an enlargement of the collateral branches.

"In the account which I have now given of the effects of the ligature on an artery, I have had in view only those instances, in which the ligature has been applied on the extremity of a divided artery, or those in which two ligatures have been applied on an artery, at a small distance from each other, and the intermediate portion divided. But, from observation on the human subject, it appears, that the effects are different, or at least their accomplishment is much more likely to be interrupted, when one or two ligatures are applied on an artery without any subsequent division of it."

The author concludes the work with some remarks on the form of the ligature, which he recommends should be of moderate size, round, and uniform, and applied with considerable firmness, so as to ensure the division of the two more internal coats of the vessel, and to prevent the ligature from being thrown off by the force of the circulation. From the toughness of the external coat, it cannot be cut through without considerable difficulty, an accident which the older surgeons very much dreaded. It is hardly necessary now to state, that the artery should be tied as clear as possible from contiguous substances, particularly large nerves. For some time after securing a large vessel, the limb ought to be kept as quiet as possible, hemorrhage having been caused as late as the twenty-first day, by sudden and violent exertion.

In analyzing European publications it is not our intention so much to pass judgment on their merits or defects, as to lay before our readers what appears to us new or important in them, but we cannot take our leave of the work before us without again expressing our high opinion of its value. It is, in our estimation, a perfect model of the manner in which physiological inquiries ought to be conducted; and had truth been thus always sought, how different would have been the state of medical science. Had physiologists, instead of indulging their fancies in idle dreams about the functions of parts which are perhaps inscrutable, certainly unimportant,—had they, like Dr. Jones, coolly inquired, patiently observed, and candidly related, we should not now have occasion so often to exclaim against the uncertainty or inefficacy of the healing art. Happily for mankind, this rage for explaining every thing seems to be passing away; physicians at last are not ashamed of being ignorant of what perhaps is inexplicable; and medical authors are now content with pointing out the method of curing diseases, instead of bewildering their readers and themselves in the mazes of hypothetical nonsense.

A Treatise on the Anatomy, Pathology, and Surgical Treatment of Aneurism, with Engravings. By ANTONIO SCARPA, *Professor of Anatomy and Practical Surgery in the University of Pavia, &c. Translated from the Italian, with Notes. By* JOHN HENRY WISHART, *Fellow of the Royal College of Surgeons, &c.* 8vo. pp. 504. *Edinburgh.* 1808.

THIS work appears to have effected an almost entire revolution in the opinions of physiologists and surgeons, as to the proximate cause of aneurism; and even the most sceptical must allow that what has been denominated true aneurism, that is, an aneurism from a distention of the coats of an artery, is a very rare disease if indeed such cases ever occur. For our author has incontestably proved, that in a great majority of instances at least, whether the disease be in the aorta or its contiguous branches, or in the limbs, whether the cause be internal, or whether the vessel has been injured by accident, that a solution of continuity in the coats of the vessel uniformly takes place, and that the sac is formed by the cellular sheath and membrane surrounding the artery.

The following is the account which our author gives of the formation of thoracic aneurisms from internal causes.

> " § 13. The phenomena which are observable in the artificial distentions of the thoracic or abdominal aorta, present themselves likewise, in my opinion, in the case of a morbid degeneration of the internal coat of the artery, during which, that coat becomes in some places weak, or very rigid and friable, and is thinned, separated, or ruptured, by the repeated jets of blood thrown from the heart. The internal coat of an artery being ulcerated or lacerated from a slow internal cause in some point of its circumference, the blood impelled by the heart begins immediately to ooze through the connections of the fibres of the muscular coat, and gradually to be effused into the interstices of the cellular covering, which supplies the place of a sheath to the injured artery, and forms, for a certain space, a kind of *ecchymosis*, or *extravasation of blood*, slightly elevated upon the artery. Afterwards, the points of contact between the edges of the fibres of the muscular coat being insensibly separated, the arterial blood penetrating between them, fills and elevates, in a remarkable manner, the cellular covering of the artery, and raises it after the manner of an incipient tumour. Thus, the fibres and layers of the muscular coat being wasted or lacerated, or simply separated from each other, the arterial blood is carried with greater force, and in greater quantity than before, into the cellular sheath of the artery, which it forces more outwards; and, finally, the divisions between the interstices of the cellular coat being ruptured, converts it into a sac, which is filled with polypous concretions, and with fluid blood, and at last forms, properly speaking, the aneurismal sac; the internal texture of which, although apparently composed of membranes placed one over the other, is, in fact, very different from that of the proper coats of the artery, notwithstanding the injured artery, both in the thorax and in the abdomen, as well as the aneurismal sac, is covered externally, and enclosed within a common smooth membrane.

"§ 14. In the very considerable number of aneurisms of the arch, and of the thoracic and ventral trunk of the aorta, commonly regarded by medical men as *true* or *incysted*, or as formed by a dilatation of the proper coats of the great artery, which I have had an opportunity of examining, I have not found a single one, in which the rupture of the proper coats of the artery was not evident, and in which, consequently, the aneurismal sac was produced by a substance completely different from the internal or muscular coat of the injured artery. To ascertain the truth, and confirm the constancy of this fact, it is not necessary that one should be possessed of uncommon knowledge in the art of dissection, but only that he be disposed to see things as they really exist, and that he undertake to examine the aneurism in the situation in which it is found, and without removing the parts which surround it, or at least that he do this with due circumspection. For, as I have asserted above, very often the examination which is made by medical men, of internal aneurisms in the dead subject, consists of little more than a simple division of the fundus of the tumour, without paying attention to the cellular sheath which surrounds the artery, above and below the place of the aneurism, and without examining the disposition and particular characters of the proper coats of the aneurismatic artery, and comparing it with the substance which forms the parietes of the aneurismal sac; and what is still worse, the aneurism is examined by some, after being removed from the body, and filled with some substance, or dried; in which preparations, there is nothing to be seen but confusion and obscurity, in every thing which relates to the true nature and structure of the parts by which the tumour is formed.

"§ 15. A circumstance very important to be known relative to this subject, has been hinted at above; viz. that the aneurismal sac never comprehends the whole circumference of the injured artery, but only a portion of the arterial tube, to which the tumour is united on the one or the other side. At this place, the aneurismal sac presents, as it were, a species of constriction, or neck, beyond which the sac of the aneurism is more or less enlarged, or expanded, and sometimes to an enormous degree. This circumstance would never accompany aneurism, or rather, quite the contrary would be found, if the aneurismal sac were produced by an equable distension of the tube and of the proper membranes of the aneurismatic artery. For, in incipient aneurisms, at least, the greatest effect of the distention acting upon the tube of the artery, the greatest size of the tumour ought to be in the artery itself, or in the beginning or root of the tumour, and the least at its fundus. But observation demonstrates, that, whether the aneurism be recent and small, or of long standing and large, the passage from the artery is always narrow, and the fundus of the aneurism, the farther it is removed from the artery, the more it is enlarged. Another circumstance worthy of attention on this head, which I have likewise pointed out above, is, that the aneurismal sac is always covered by the same soft distendible cellular substance, which in the sound state surrounded the artery, and united it to the adjacent parts; which soft cellular substance, supposing it to be an aneurism of the arch, or of the thoracic trunk of the aorta, is covered by the pleura, and if the aneurism is in the abdomen, by the peritoneum; which membranes include the aneurismal sac, together with the ruptured artery, and present externally, a continued, smooth, shining surface, as if the artery alone was in that way dilated.

"§ 16. But if, instead of dividing, as is commonly done, the fundus of the aneurismal sac, the aorta be divided lengthwise on the other side, and opposite to the constriction or neck of the tumour, the place of the ulceration, or of the rupture of the proper coats of the artery, immediately appears within the artery, on the side opposite to that where the incision was made, and the fissure which has taken place is immediately discovered, the edge of which is sometimes fringed, often callous and hard, like that of a fistula; through which fissure the arterial blood had formed itself a passage into the cellular sheath of the artery, afterwards converted into an aneurismal sac. If, as sometimes happens in the arch of the aorta in the vicinity of the heart, the artery, before being ruptured, has suffered some degree of enlargement beyond its usual diameter, it appears at first sight that there are two aneurisms; but the constriction or neck which the aneurismal sac next to the artery presents externally, points out exactly the limits, beyond which the internal and muscular coat of the aorta had not been able to resist the distention, and have therefore been torn by it, and shows clearly the difference existing between an aneurism and a simple enlargement in diameter of the tube of the aorta in the vicinity of the heart.

"§ 17. The rupture in the artery is always small in proportion to the large size of the aneurismal tumour; so that when the arch of the aorta has suffered some degree of dilatation before bursting, as sometimes happens near its passage out of the heart, on making an incision on one side into the aneurismal sac, and on the other into the tube of the artery lengthwise, two sacs present themselves, separated from each other by means of a partition or diaphragm lacerated in its middle; which partition is formed of nothing else than the remains of the internal and muscular coats of the ruptured artery. And as the limits of the proper coats of the aorta, and the beginning of the cellular aneurismal sac, are marked externally by that kind of constriction or neck which the tumour presents in the vicinity of the artery; in the same manner, internally, this partition, torn in its middle, determines the precise point of the rupture of the proper coats of the artery occupied by aneurism.

" § 18. All this acquires a degree of demonstration and certainty, to which nothing can be opposed, by carefully dissecting the proper coats of the ruptured aorta in its situation, and comparing them at the same time with the cellular substance forming the aneurismal sac; for, in the incision made in the direction of the axis of the aorta, and in its side opposite to that where the rupture has taken place, its proper coats are found either perfectly sound, or a little weakened and intermixed with earthy points, but still capable of being separated distinctly into layers from one another; when, on the contrary, in the opposite side of the aorta, where the ulceration or laceration exists, its proper coats are met with unusually thin, blended together, and with difficulty, or in no way capable of being separated from each other, intermixed very often with heterogeneous substances, which render them brittle like the shell of an egg; and, lastly, disorganized and torn at the place where they form that species of partition, which marks the limits between the ruptured artery and the entrance of the aneurismal sac. Continuing to separate these coats from within outwards, we come to the cellular sheath which surrounds the aorta externally. Then, on removing the cellular pulpy sheath of the aorta, it is found smooth externally, like the artery, villous, cellular, and irregular internally, extending from the circumference of the tube of the artery over the neck and fundus of the aneurismal sac. This external covering or sheath of the artery actually appears, to those who are not sufficiently skilled in such dissections, as if the artery were dilated under it to such a degree as to form the aneurism; and it has still more that appearance if the aneurism is very large and of long standing, since in this case the cellular sheath of the artery becomes unusually thick and pulpy, and because it adheres very firmly to the subjacent muscular coat of the artery at the stricture or neck of the aneurismal sac. But even in these cases, as well as in those of recent and small aneurisms of the aorta, by employing care, we may at last succeed in separating, without laceration, this cellular sheath from the tube of the artery, above and below the injury, and successively from the subjacent muscular coat, as far as the neck or root of the aneurism. It is then clearly perceived that the muscular coat of the aorta does not pass beyond the partition which divides its tube from the entrance of the aneurismal sac; and it is distinctly observed, that the fibres and layers of the muscular coat are not prolonged over the aneurismal sac, but terminate like a fringe, or in obtuse points, at the edge of the rupture of the artery. On which account, nothing can be more evident than that the aneurismal sac does not belong at all to the artery, and that, properly speaking, it is only the cellular sheath, which in the sound state covered and connected the artery to the neighbouring parts, which, being elevated by the effused blood, at first in the manner of an *ecchymosis*, then distended and compressed, has acquired that degree of density, and of additional hardness and thickness, as if it had been formed by the proper coats of the artery, prodigiously relaxed, distended, and thickened. These appearances the more readily lead to error, as both the injured artery and the aneurismal sac, as has been frequently mentioned, are covered by a common smooth membrane externally, such as the pleura in the thorax, and the peritoneum in the abdomen.

" § 19. The favourers of the contrary opinion do not deny that the artery is sometimes ruptured in internal aneurisms, but they only admit this in those cases in which the dilatation of the artery has been carried to a prodigious extent. The invalidity of this opinion appears to me to be clearly proved by the rupture of the artery being constantly met with, whether the internal aneurism be small or large, and the artery be a little, or, as most frequently happens, not at all dilated. And this opinion is likewise contradicted by its being now ascertained, that the internal and muscular coats of the artery terminate evidently in the fringe, or hard margin of the orifice of the partition, and that the aneurismal sac is not at all formed by the proper coats of the artery, but by the cellular sheath which surrounds it. In the large aneurisms in which the partisans of the common doctrine admit the rupture of the artery, a double rupture is always found, the one of the artery, the other of the aneurismal sac; and this last is that which actually kills the patient, by changing the aneurism from the *circumscribed* into the *diffused* state. The erroneous opinion, that the large arteries, and especially the trunk of the aorta, are as it were insulated, or covered only by a very fine membrane, adhering firmly to the subjacent muscular coat, and easily lacerated, has given rise in all probability to the belief, that the ulceration or rupture of the internal proper coats of the aorta could not take place, without this thin tense membrane covering the artery externally being likewise torn. Undoubtedly, if this were the case with regard to the external cellular sheath of the arteries, it would necessarily follow, that the blood should be effused into the thorax and abdomen, in every case of rupture of the abdominal or thoracic aorta. Indeed, we sometimes see this happen in practice, by the combination of some particular circumstances; for, if unfortunately the rupture of the internal and muscular coat of the aorta happens in such a point of the artery, where externally there is only a thin tense membrane closely applied to the aorta, instead of a corresponding soft cellular sheath, we see that this external thin membrane is likewise comprehended in the rupture of the proper coats of the artery, and that therefore sudden death takes place, from the inevitable effusion of blood into some of the principal cavities, especially the thorax. Such is the case whenever the internal and muscular membranes of the aorta are ruptured in that portion of the artery included within the pericardium, which is only covered by a thin reflected layer of it. In this particular circumstance of place and structure, as soon as the proper coats of the aorta are ruptured, this thin membrane is lacerated at the same time, and the blood is immediately effused into the cavity of the pericardium. An example of this kind is related by Walter, and illustrated by a figure; by the Berlin Physicians; and several other fatal accidents similar to these are recorded by Morgagni. I have likewise observed a case similar to the preceding, the history of which I think proper to relate here. Joseph Varani, 22 years of age, a corporal of the 4th company of pioneers, while he was conversing cheerfully with his companions, was struck suddenly dead. This man, formerly by trade a shoemaker, had been repeatedly infected with lues venerea, and had also several times undergone a mercurial course. He had never, however, complained of difficulty of breathing, and his pulse had never been found irregular or intermitting, not even a few weeks before his death. On examining his body, the pericardium immediately presented itself quite distended with blood. The aorta in the vicinity of the heart, at the distance of half an inch above its valves, where it began to be incurvated, presented externally a tumour of the size of a nut, which opened by a small hole within the pericardium.

The reddish, and, as it were, ecchymosed membrane, of this tumour diminished in thickness in proportion as it approached the place of the rupture, round which it was very thin. On making an incision into the concave part of the aorta, on the side opposite to the seat of the tumour, its internal coat, where it corresponded to the base of the tumour, was quite rough, corroded, interspersed with yellow hard spots, and actually *ulcerated* for the space of an inch in circumference. In the centre of this ulcerated tract, both the internal and muscular coats of the artery formed a slight pit, into which the point of the fore-finger could be introduced, so that any one, who had contented himself with the appearance of the parts, would have said, that this small tumour, which appeared externally on the beginning of the curvature of the aorta, was formed solely by the weakened and distended proper coats of the artery, or that this tumour was a *true* aneurism. But on removing carefully from the curvature of the aorta its external cellular sheath, and consequently the reflected lamina of the pericardium, leaving the subjacent fibrous coat untouched, I found that this reddish and ecchymosed sac was formed solely by the cellular sheath, and by the reflected lamina of the pericardium. This cellular capsule, at its base, adhered very firmly to the irregular edges of the ulceration, and rupture of the proper coats of the aorta, in such a manner that the limits of the one and of the other could be clearly distinguished. On which account, in this case there were two ruptures, as in all the others of sudden death caused by aneurism, the one in the proper coats of the artery, the other in its external cellular capsule. The thin, tense, reflected lamina of the pericardium, being provided with very little soft cellular substance under it capable of extension, when raised to the size of a nut, not being disposed to yield any further, burst,* and allowed the blood to escape into the cavity of the pericardium. But this is not the case in the other parts of the aorta, in its curvature without the sac of the pericardium, nor in its thoracic and abdominal trunks; through all which tract the cellular sheath of this artery, far from being a fine tense pellicle, adhering firmly to the muscular coat, as within the pericardium, is, on the contrary, a pulpy weak covering, allowing itself to be easily distended. Whence it happens that this cellular sheath is well fitted for yielding to the impulse of the arterial blood which is effused, and, by yielding, it allows itself to be formed into a sac, which, strengthened internally by the addition of the polypous layers, and externally and anteriorly by the pleura in the thorax, and the peritoneum in the abdomen, opposes, sometimes for a very considerable time, the sudden fatal effusion of blood into these cavities.

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* The pathological preparation mentioned here is preserved in the museum of this university.

"§ 20. Of all the causes capable of producing the rupture in any part of the proper coats of the aorta, especially the internal, I have great reason to believe, that the slow morbid, *ulcerated*, *steatomatous*, *fungous*, *squamous* degeneration of the internal coat of the artery, has a share in it much more frequently, than violent exertions of the whole body, violent blows, or an increased impulse of the heart. The artery is nourished, and increases in the same manner as all the other parts of the animal body; it is vascular and organized; and therefore must be subject to the diseases to which vascular and organized parts are liable. And it is a fact, of which no doubt can be entertained, that the proper coats of the aorta, and especially the internal coat, are subject, from a slow internal cause, to an *ulcerated* and *steatomatous* disorganization, as well as to a *squamous* and *earthy* rigidity and brittleness."[*]

The following case and dissection are given among many others in proof of the author's doctrine.

"Pasqual Castiglioni, 45 years of age, of a stout but slender habit of body, addicted to the pleasures of wine and women, having served 25 years in the Austrian army, came to the hospital the 5th of November, 1801. He said, that he had been subject, for about six months, to almost constant pains in the lumbar region, stretching over the abdomen.[†] The pains were more violent during the night than in the day time, and were accompanied by difficulty of breathing, languor, and ardor urinæ. The urine was quite cloudy, of a dark red colour, with a chalky sediment, and in the moments of greatest spasm the tension and weight extended to the testicles, especially to the left. For four months after the attack of pain, this patient had not had recourse to any method of cure, but trusting entirely to his own knowledge, took some rhubarb and diuretics, and had his loins stung with nettles; after which, for eighteen days the diurnal pains seemed to have been rather relieved. The pains recurring again, he put himself into the hands of a physician, who conceived the disease to be rheumatism; in consequence of which, he made him apply blisters, and use sudorific decoctions, which only caused a greater exacerbation of his disease. Another physician was consulted, who supposing that there were calculi in the kidneys, prescribed for him the volatile liniment, soap pills, and emollient decoctions, but all without any advantage. Indeed, there were none of the proper symptoms of nephritis present. A fixed pain extending to the left lumbar region; an almost continual restlessness; a regular pulse; no fever; no vomiting; the lower extremities sometimes quite cold, while the upper extremities, and especially the head, perspired copiously, from the violence of the pains, which attacked him for the most part in the afternoon, or during the night, and extended to the left lower false ribs, to the loins, to the left thigh, and sometimes to the left testicle. These pains were more acute in rainy or windy days, than in serene weather. Opium alone, in large doses, combined with assafœtida, and gentle friction with oil of amber, procured him some relief. The patient preferred lying on his left side, or bent forwards when he sat up in bed. No pulsation was felt in the left lumbar region, either by the patient or by the assistants. This unfortunate man, in the morning of the 15th February, 1802, was found dead in his bed, lying on his left side.

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[*] Schulzius mentioned, that he had seen in a mare, the artery which runs along the colon, occupied by aneurism, the centre of which was full of small worms. Acta nat. curios. vol. i. obs. 219, p. 519. The tumour was of the size of a large nut, hard, and of an oval figure. A probe passed readily along within the artery, but at the place of the tumour, it seemed to pass through a ball of hair or wool. On opening the artery lengthwise, the author found, that its coats, at the place of the tumour, were three times thicker than natural. On examining the inside of the tumour formed by the thickened coats of the artery, he observed, that its substance was quite cellular, and that the cells were full of capillary worms twisted together.

[†] See a case very similar to this described by Sir John Pringle, Essays and Obs. Physical and Literary, of Edinburgh, t. iii. p. 200; ibid. Monro, case x. page 225; Walter. Mem. de Berlin, an. 1785, p. 56. tab. i. ii.

"On opening the abdomen, and removing the intestines, there appeared along the left lumbar region, as far as the crural arch of the same side, a black coloured swelling, apparently gangrenous, which appearance was however only caused by black extravasated coagulated blood behind the peritoneum. On examining the aorta from the heart downwards, in its proper situation, when I came to about eight fingers-breadth before the thoracic aorta passes under the crura of the diaphragm, the aneurism presented itself resting on the bodies of the lower vertebræ of the back, and I then ascertained the true nature of the disease.

"Having evacuated this great collection of extravasated grumous blood from the abdomen, by which the peritoneum had been raised from the appendices of the diaphragm, as far as the left crural arch, I remarked that the effused blood had formed deep excavations in the substance of the left psoas muscle, so as to disorganize the lumbar nerves, and injure the *anterior crural* and *obturator* nerves. This accounted for the very acute pains felt by the unhappy patient in these parts, and for the sense of coldness which he experienced in them. From these deep pits, which had been filled with grumous blood, my fingers passed freely behind the peritoneum, between the appendices of the diaphragm within the thorax, behind the pleura, and from thence into the cavity of the aneurism, which rested, as I have said, upon the inferior vertebræ of the back, and was full of grumous blood. The thoracic aorta, examined on its anterior surface, seemed to pass over the aneurism, without having any share in the formation of it; and the more so, as this artery had preserved its natural diameter in its whole course. I submitted the particular conformation of this aneurism to a careful examination. Having raised the pleura which covered the trunk of the thoracic aorta, as well as the aneurism anteriorly and laterally, a thick pulpy cellular substance presented itself under the pleura. Having separated slowly the cellular sheath which surrounds this artery in the natural state, all round the thoracic aorta, for a considerable way above and below the aneurism, I saw plainly that it was this pulpy cellular substance, this natural external sheath of the artery, quite distinct and capable of being separated from the subjacent muscular coat, which being raised by the effused arterial blood, had been converted into an aneurismal sac. Having then made a longitudinal incision into the thoracic aorta, on its anterior surface, and in that space where this artery seemed to run uninjured over the aneurism, there immediately appeared opposite to the incision, or in the posterior side of the artery, a rupture a little more than an inch in diameter, with irregular, hard, callous edges; through which rupture my finger passed into the large aneurismal sac. The tube of the thoracic aorta was not at all dilated at the place of the rupture. The edges of the laceration still preserved the characters of the proper coats of the artery, especially of the muscular coat, and there was an evident difference, at the edges, between the truncated extremities of the fibres of the muscular coat, and the beginning of the cellular pulpy aneurismal sac, over which, as I have mentioned, the pleura extended, as well as over the trunk of the thoracic aorta. Having then turned the thoracic aorta along with the aneurism, from the left to the right side of the vertebral column, I found that the aneurismal sac, on the side next to the bodies of the inferior vertebræ of the back, was wanting for a certain space, and that there the place of the sac was supplied by a portion of the bodies of the dorsal vertebræ, partly corroded, with the exception of the intermediate cartilages, which, as other surgeons have remarked, remain in similar circumstances, uninjured longer than the bony substance. On examining attentively the internal surface of the posterior part of the aneurismal sac, or that which was next to the vertebral column, there appeared in the bottom of the sac, a rent, or passage from the posterior side of the thoracic aorta into the aneurismal sac, and this posterior side of the artery, resembling a partition perforated in its middle, marked the boundaries between the proper coats of the artery, and the beginning of the cellular aneurismal sac. The trunk of the thoracic aorta, examined also on its posterior surface, was not at all dilated beyond its natural diameter. Its internal coat, round the place of the rupture, had lost its natural smoothness, and was here and there covered with earthy scales, and ossified points; which appearance was also observed in this patient, on the internal coat lining the curvature of the aorta, in the vicinity of the heart."

The author gives the following summary of his doctrine.

" § 37. From all that has been hitherto said with regard to aneurism in general, and more particularly of that of the aorta, it appears to me, that we may with certainty conclude, 1st, That this disease is invariably formed by the rupture of the proper coats of the artery. 2dly, That the aneurismal sac is never formed by a dilatation of the proper coats of the artery, but undoubtedly by the cellular sheath which the artery receives in common with the parts contiguous to it; over which cellular sheath the pleura is placed in the thorax, and the peritoneum in the abdomen. 3dly, That if the aorta immediately above the heart appears sometimes increased beyond its natural diameter, this is not common to all the rest of the artery; and when the aorta in the vicinity of the heart yields to a dilatation greater than natural, this dilatation does not constitute, properly speaking, the essence of aneurism. 4thly, That there are none of those marks regarded by medical men as characteristic of aneurism from *dilatation*, which may not be met with in aneurism from *rupture*, including even the circumscribed figure of the tumour. 5thly, That the distinction of aneurism into *true* and *spurious*, adopted in the schools, is only the production of a false theory; since observation shows, that there is only one form of this disease, or that caused by a *rupture* of the proper coats of the artery, and an effusion of arterial blood into the cellular sheath which surrounds the ruptured artery."

Had the author detailed his own dissections only we should be disposed to think he might have deceived himself, or that his cases were singular; but he has collected, with great industry, all the authorities, ancient and modern, which bear upon his subject, and has thus accumulated such a weight of testimony as to insure conviction.

Aneurisms in the extremities, from internal causes, are formed in the same manner as those in the thorax, except the variation arising from the difference of parts by which the vessels are surrounded.

Instead of dividing aneurisms into true and false, our author more properly distinguishes them under the appellations of circumscribed and diffused: the former comprehending all those aneurisms having distinct sacs; the latter those cases in which the blood is injected into the cellular membrane, distending it to great extent. In the former case there are two methods of curing the disease, one by ligature, the other by means of pressure. There are few cases in which the latter remedy can be employed, and it is, we believe, rarely if ever practised by the British surgeons. In tying an artery which has been wounded, our author recommends the surgeon to follow the course of the wounding instrument, and to introduce a probe into the aperture in the artery, in order to elevate the vessel, and to be certain of including the injured part between the two ligatures.

In the appendix the translator has added some valuable cases; in one of them mortification came on in consequence of the ligature of the artery.

A Dissertation on the Mineral Waters of Saratoga, including an Account of the Waters of Ballston. By VALENTINE SEAMAN, *M. D. one of the Surgeons of the New-York Hospital. Second Edition, enlarged. New-York.* Collins & Perkins. 1809. pp. 131.

THE first edition of this pamphlet was printed about sixteen years ago. The public, therefore, have had abundant time to judge of its contents; and it is not our business now to pronounce on its merits. We shall notice only some of the material alterations and additions made by the author in this second edition.

To his introduction he has added an amusing paragraph or two, in which he compares and contrasts the *cold boiling springs of Saratoga*, and the *burning mountains of Italy*—the *furious flames and torrents of lava* issuing from a volcano, and the gentle bubblings of these *refreshing fountains*. These two objects, he says, are *vastly different;* in which, no doubt, every one will agree; but that they are *equally curious* the common sense of mankind may not so generally admit. This comparison, however, is full of expression. It shows, among other things, how much authors are inclined to *magnify* the favourite objects of their research. We imagine it must have been conceived by Dr. S. after he had drawn from memory on his map (which is also an addition to this edition) the shape of the conical covering of the spring; for, as it is there delineated, it certainly resembles more the crater of mount *Vesuvius* than it does the *rock spring* of Saratoga. The real covering of the spring, if we mistake not, is so flattened, and the declivity of the cone so gentle, that persons easily walk on its sides, and stand around the opening. The drawing, therefore, is materially incorrect, and conveys an erroneous idea of the object—the base of the cone is represented, proportionally, too small, and the elevation much too great.

All that part of this edition which relates to the Saratoga waters is little more than a transcript of the former publication, except that to the list of ingredients contained in the waters he has added *carbonated magnesia.* In his first pamphlet he had asserted that they contained no magnesia. He has now concluded, it appears from the experiments he formerly made, that they do contain this ingredient, but [illegible] what quantity he does not mention.

The next article relates to the *Ballston* springs, as they are termed by the author. These springs have, no doubt, received their names from the *town* in which they are situated; and the town, we always understood, received its appellation from a clergyman by the name of Ball, who was one of the first settlers there, and whose house is still standing near the road, a few miles from the springs. The true name, therefore, however it may be used in subserviency to the "*ton of the times*," is *Ballstown.* But this is a matter of sound; we proceed now to the substance.

The principal object of what the author has here written, he tells us, is to decide the question of difference between an analysis formerly made by Dr. Vandervoort, and afterwards by a French chemist, and also to ascertain the real contents of the waters. These two analyses, Dr. S. observes, differ first with respect to the quantity of carbonic acid, or fixed air. The French chemist says the water contains *three times its bulk* of this air; whereas Dr. Vandervoort could not obtain from it more than *one third part* of that quantity. Dr. S. is inclined to believe the latter of these correct, because neither Berghman nor Henry could cause water to absorb more than that proportion. But it may be urged on the other side of the question, that perhaps the pressure to which the waters of these springs are exposed in their confinement under the earth, may be ten or a hundred times greater than either Berghman or Henry could give them. Thomson expressly says, that "*some of the ærated alkaline water prepared in Britain as a medicine, is said to con-*

tain about thrice its volume of carbonic acid gas."* And we have reason to believe the Ballstown springs more highly impregnated with this air than any artificial water can well be. As this question, therefore, remains perfectly doubtful after all that has been said on this subject, we wish Dr. S. had endeavoured to settle it by actual experiment; for it ought to be remembered, that mere reasoning can never decide a matter of fact.

With respect to the next point of difference, Dr. S. proceeds as he ought to have done in the first. He tries his own skill at analysis. But here too he appears to have failed. The French chemist says the water contains muriate of lime, and Dr. Vandervoort makes no mention of that composition. Dr. S. dropped in the water, previously evaporated to one fourth its quantity, some oxalic acid, which produced no *cloudiness* or *precipitation*; whence he concludes no *muriate of lime* exists in it; but this experiment, supposing no error in it, proves too much, for it also proves that there was no *lime* in the water in any shape, which both Dr. S. and Dr. Vandervoort enumerate among its ingredients; and boiling alone could not have extricated it. The "*oxalate of lime is altogether insoluble*," and the "*oxalic acid is capable of taking lime from every other acid.*"† It seems, therefore, that our author's experiment is no better than his reasoning, there being an evident fallacy in both.

But there is a third point of difference which Dr. S. attempts to decide, partly by experiment, and partly by reasoning. The French chemist asserts that the water contains *muriate* of magnesia. Dr. S. from the effects of re-agents on the waters, concludes they contain *carbonate of soda*; and if so, he says it is *impossible* that the muriate of magnesia should exist in them, for the superior attraction of the alkali for the muriatic acid would utterly prevent the formation of a magnesian muriate. This argument, however, is not so conclusive as he seems to think; for if he will examine the modern discoveries concerning chemical affinity, he will find that there is such a thing as an acid dividing itself between several alkalies and earths in different proportions, without, perhaps, completely saturating either of them, and, consequently, forming several salts in the same mixture, which yet may exhibit alkaline properties. Thus it many be in the present instance. Although the soda may not be neutralized by the muriatic acid, yet small portions of that acid may at the same time be combined with the lime and the magnesia: at all events, as the doctrines of affinity are not yet perfectly established, nor well understood, the examination of mineral waters by chemical tests alone is always liable to error; and it will be doing no great violence to the author's experiments to say, that no certain conclusions can be drawn from them. In short, we cannot conceive that he has contributed any thing towards settling the difference he proposed; and the account he has given of the contents of the water is extremely imperfect, in so far, at least, as the quantity of any of the ingredients is not mentioned. In this chapter, therefore, we fear he has not succeeded in accomplishing any of the desirable objects he proposed to himself.

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* In preparing the soda water in this city, we are informed that Mr. Usher, by an invention of his own, applies so great a pressure as to condense ten gallons of air in one gallon of water.

† Thomson's Chemistry.

In that part of the work which treats of the medical virtues of the Ballstown and Saratoga waters, he has made some additions, principally derived from information received from Dr. Powell, who resides at the springs: and to the whole he has annexed *considerations on the use of the artificial mineral waters as prepared in this city.* Some of his observations here are judicious; but we cannot help regretting that what he has said of the soda waters, may have a tendency to discourage their extensive use. He seems to imagine some unwholesome quality lurking in the minute portion of alkali they contain. In this we think few will agree with the author. The truth rather appears to be, that in a city like this, where luxury and intemperance prevail, a large proportion of the inhabitants occasionally complain of acidity, from defect in their digestive powers; and no one general remedy could probably be devised so well calculated to correct the pampered appetites of the people, and to promote health and temperance, as these delightful waters.

Notwithstanding the critical remarks we have been compelled to make on some parts of this pamphlet, we wish not to be understood as casting a censure on the work generally; we wish rather to encourage than to discountenance investigations of this kind, but we wish also to see them well executed. The author of the publication before us certainly deserves great credit for the labour he has bestowed on this subject, and particularly for collecting information concerning the medical virtues of these waters. This information was much wanted, and will undoubtedly be useful. The whole performance, indeed, is well calculated to inform and to amuse the passing traveller who has not had an opportunity to see and to taste for himself. This, we presume, was the principal design of the writer, and thus far his design will be accomplished. The man of science assuredly cannot drink deep of this pierian fount, nor will he find it either pungent to his taste, or exhilarating to his mind; but, like the plain and wholesome water of ordinary springs, it may quench the public thirst, though it do not brighten the public intellect.

Essay on Sheep; their Varieties—Account of the Merinoes of Spain, France, &c. Reflections on the best method of treating them, and raising a Flock in the United States: together with Miscellaneous Remarks on Sheep and Woollen Manufactures. By ROBERT R. LIVINGSTON, *LL. D. President of the Society for the Promotion of Useful Arts, &c.* pp. 186. *New-York.* T. & J. Swords. 1809.

IT is impossible to do justice to the great merits of this work in a review. It may confidently be pronounced one of the most valuable American publications ever issued from the press.

Every farmer in the country ought to possess it, and make himself master of its contents. Referring therefore to the book itself, we can only gratify our readers with a quotation from the author, respecting the Merino sheep, and the manner in which the travelling flocks in Spain are conducted.

"I come now to speak of those breeds of sheep that are best known to us, and indeed the most useful in our state of society—the sheep of Europe. I should however first observe, that some provinces of Persia possess a breed of sheep whose wool is finer and more valued than that of Spain; but as I have no where met with a minute account of them, I shall proceed to notice the race of sheep which holds the first rank, and bears the finest fleeces of any known in Europe—I mean the Merino sheep of Spain. The race varies greatly in size and beauty in different parts of Spain. It is commonly rather smaller than the middle sized sheep of America. The body is compact, the legs short, the head long, the forehead arched. The ram generally (but not invariably) carries very large spiral horns, has a fine eye and a bold step. The ewes have generally no horns. The wool of these sheep is so much finer and softer than the common wool, as to bear no sort of comparison with it; it is twisted and drawn together like a cork-screw; its length is generally about three inches, but when drawn out it will stretch to nearly double that length. Though the wool is, when cleaned, extremely white, yet on the sheep it appears of a yellowish or dirty brown colour, owing to the closeness of the coat, and the condensation of the perspiration on the extremities of the fleece. The wool commonly covers great part of the head, and descends to the hoof of the hind feet, particularly in young sheep; it is also much more greasy than the wool of other sheep. Spain contains besides the Merinoes, a variety of other sheep. Those called the Choaroes are much longer, larger, and higher upon the legs than the Merinoes. Their heads are smaller and deprived of wool. Their constitutions are stronger than those of the Merino. Their wool near eight inches long, but straighter and of less value than that of the Merino. This race extends through all Spain, even into those provinces in which the Merino is most perfect. The other sheep are a mixed breed between those and the Merino. The number of these two species is computed at about 6,000,000. Among the Merinoes there are varieties, probably occasioned by the care or fancy of the original cultivators of this valuable stock in different parts of Spain. Castile and Leon has the largest, with the finest coats. Those of Soria are small, with very fine wool. Those also of Valencia, which, like the last, do not travel, have fine wool, but of a very short staple. The greater part of the Merinoes of Spain are transhumante, and migrate from the south to the north, and from the north to the south twice every year. This has probably contributed to the health of the sheep, and, as a consequence of it, to preserve the beauty of the wool, without however being essential to it; as appears from the fine wool produced by the stationary flocks that I have mentioned, and other stationary flocks in the hands of individuals, whose wool is not inferior to that of the migrating sheep. Spain is bounded to the north by mountains of such altitude as to be covered during the winter with snow. These however afford fine pasturage in the spring and summer, when the plains in the south are parched by the sun. It was very natural then for the shepherd to avail himself of this circumstance, and while the country was little cultivated, to drive his flock from the burnt grass of the plains to the fresh and verdant herbage of the mountains; and again, when this was chilled by frost, or covered by snow, to return to the plains that had regained their verdure; the winters of Spain not being so severe as to destroy the vegetation, except in the mountains. Necessity also contributed to keep up this practice. During the long wars that were carried on between the natives of Spain and the Moors, agriculture was neglected, and the only property which could be saved from the ravages of an enemy, was that which could be easily removed; but they were content at that time to travel only from the plains to the adjoining mountains, and not as at present to travel the whole kingdom twice a year. Neither convenience nor necessity can be offered as an excuse for a practice so hurtful to agriculture. This was founded in abuse of power. Happy would it be for mankind if this was the only instance in which tyranny and oppression had been engrafted upon necessity.

"The greater part of the travelling flocks in process of time got into the hands of the sovereign, or into those of the principal courtiers and clergy; and from thence we must probably date the oppressive code by which their march is regulated, and the origin of the great Council of the Royal Troop (Consejo de la Mesta), by whom those laws are administered. M. Lasterie, in his excellent treatise, gives the following account of this council.

"The Mesta, which originated with the times, in which force only gave law, about the middle of the fifteenth century, formed a political body. This association was composed of rich and powerful persons, and some monks, all proprietors of flocks, which, under the authority of government, made laws and decided questions relative to pasturage and flocks of sheep. Two great quarto volumes formed the code of privileges, and the arsenal in which were found arms to combat justice and oppress the weak. It was seldom that proprietors of land made demands when they sustained damage, thinking it better to suffer than to contest, when they were assured that the expense would greatly exceed any compensation they might recover. It is sufficient to say, that this tribunal is not only adverse to the enclosing of land, but that, under some circumstances, it may prohibit proprietors from cultivating their inheritance. A Spanish writer (Jovellanes), in a memoir addressed to the King of Spain, says, 'the corps of Junadines (the proprietors of flocks) enjoy an enormous power, and have, by the force of sophisms and intrigues, not only engrossed all the pastures of the kingdom, but have made the cultivators abandon their most fertile lands: thus they have banished the stationary flocks (the estantes), ruined agriculture, and depopulated the country.' It is easily conceived, that five millions of sheep traversing the kingdom in almost its whole extent, for whom the cultivators are compelled to leave a road through their vineyards and best cultivated lands of not less than ninety yards wide, and for whom, besides, large commons must be left; I say, it is easily conceived that such a flock must greatly contribute to the depopulation of the country, and that the revenue that the King draws by the duty on wool is snatched from the bread of his people."

"When the severe weather commences upon the mountains, the shepherds prepare to depart, which is generally about the end of September and throughout the month of October, to seek more temperate climates and fresher pastures. In April and May, according as the season is late or early, they return to the mountains. They generally travel about five or six leagues a day, and stop occasionally in the pastures prepared for them: the head shepherd precedes, and the rest flank or follow the flock to collect the stragglers. Like Virgil's Libean shepherds, they carry every thing with them.

> Omnia secum
> Armentarius Afer agit, tectumque, laremque,
> Armaque, Amyclæumque canem, Cressamque pharetram.

This is comprised in a very short catalogue. The skins of sheep that serve for their beds, a kettle, a leather bottle, a knapsack, a spoon, a lancet to bleed their sheep, a scissors, a hatchet, a knife, and bread and oil or suet, on which they subsist, and a few drugs for their sheep. These, with the skins of those sheep that die in the passage, are carried by a few beasts of burthen which accompany the flock. To facilitate the march, a number of wethers of the largest size, which they call Mansos, are rendered very tame. These carry bells, and are taught to obey the signals of the shepherds, and either march or stop as they direct. The rest of the flocks follow their leaders. As soon as they arrive at their winter quarters, the shepherd's first care is to form the pens in which they are gathered at night to protect them from the wolves, who always migrate with the sheep, in order to pick up the sick, the weak, or the stragglers. These folds are made of genista hispanica, which is a soft rushy shrub: mats, baskets and ropes are made of it. The meshes of these net enclosures are a foot wide. The dogs, which are of a large breed, serve to guard this fold at night. The shepherds make their own tents with stakes, branches, and brambles, and have for this purpose a right to take one branch from every forest tree. Ten thousand sheep compose a flock under the direction of one chief, and this is divided into ten tribes. The head shepherd has absolute dominion over fifty shepherds, and as many dogs, five of each being annexed to a tribe. His salary is about two hundred dollars a year, while that of the first shepherd of a tribe is only ten, the second eight, the third and fourth still less, and a boy only two and a half. Their daily allowance of food is two pounds of bread, and as much to each dog. They may keep a few goats or sheep, of which they have the meat, but not the wool. They receive as a gratuity about six shillings in April, and as much in October, by way of regale. On the road they are every day, at all seasons, exposed to the air, and at night have no shelter but their miserable huts. In this way live to a considerable age the twenty-five thousand men that compose the shepherds in Spain. The flocks consist of rams, ewes, wethers, and lambs, in the following proportion: five rams, one hundred ewes, twenty-five wethers, and fifty lambs. The small number of lambs is owing to the shepherds killing all that are not necessary to keep up their stock, which is, of course, limited by the right of pasturage. The number of travelling Merino sheep is about five millions. The fleeces of the rams weigh eight and a half pounds, of the ewes five, which loses half in washing; but in this there is a great variety, according to the different species of Merinoes. The produce is about twenty-four reals, or sixteen shillings per head. Of this the owner receives but two, the King six, and the remainder goes to the payment of expenses, of pasture, tythes, shepherds, dogs, &c. When the sheep return to their summer pasture they have as much salt given them as they will eat. One thousand sheep are allowed one hundred pounds of salt, which they eat in about five months. They eat none when on their journey, or in their winter quarters. They suppose in Spain that salt contributes greatly to the fineness of the wool. The shepherd places fifty or sixty flat stones at about five paces from each other; he strews salt upon, and leads the sheep among them. In the month of April it requires some vigilance to prevent the sheep from marching off without their shepherds, to the very place where they fed the preceding year, which they sometimes do to the number of three or four hundred in a flock."

INTELLIGENCE.

DOMESTIC.

Medical School of New-York.

College of Physicians and Surgeons in the City of New-York,
August 7th, 1809.

THE public has already been informed that the Regents of the University, who are appointed by the Legislature, with a superintending power over all the incorporated seminaries of learning in the state, established, in the year 1807, a College of Physicians and Surgeons in the city of New-York, under their immediate direction, with a particular view to the advancement of medical knowledge.

After the first session, during which the members of the College and the professors endeavoured, with unremitting industry, to discharge the duties assigned them, and to fulfil the expectations of the public, the Legislature honoured the College with a signal mark of its favour, in the grant of a liberal endowment; thus insuring the stability of the institution, and enabling it to provide those expensive appurtenances, without which a course of medical lectures would necessarily remain incomplete. Henceforth this College will be found provided with all that is requisite for the most liberal medical education.

The New-York Hospital is regularly attended by three of the professors, under whose immediate directions the students of physic will have access to an excellent school of practical instruction. This institution usually contains more than 300 sick persons; and Clinical Lectures on the cases occurring there are given by the professor of the practice of physic. The opportunities to acquire surgical knowledge are very ample, both in the practical and operative branches. The extensive medical library of the Hospital will also furnish the student with many important resources, as well with regard to the value of the works it contains, as the facility of consulting them.

In the Anatomical department no exertions will be wanting to provide the most ample means of instruction; the professor of anatomy devoting nearly the whole of his time to the duties of his station.

The cabinets of Natural History and Mineralogy, belonging to the professors of those branches, are already very extensive, and used for demonstration in their lectures. In these collections, which are almost daily increasing, the geological constitution and fossil resources of the United States are particularly displayed.

The course of Chemistry will embrace a particular account of all the new and important discoveries in that interesting department of knowledge, together with an exhibition of the experiments necessary to illustrate them, as well as the fundamental principles of the science.

The several courses of Lectures will commence regularly on the first Monday in November, for the winter sessions; and on the second Monday in April, for the summer sessions, in every year: the former will continue about four months, the latter about three.

A fee, not exceeding fifteen dollars, will be required from the student for attending each course of lectures, except those on anatomy, which, on account of the extraordinary labour and expense required in that department, will be twenty dollars.

The President of the Medical Society in every county of the state, is invited to designate a Student of Medicine, of fair moral character, of promising talents, and diligent habits, who shall be admitted to attend the Public Lectures in the College free of expense.

Medical honors will be conferred by the University of the State on such students as give satisfactory testimony of their proficiency in the several branches of education connected with the healing art.

The Professors appointed by the Regents are, at present,

Nicholas Romayne, *M. D. President, and Professor of the Institutes of Medicine.*

Samuel L. Mitchill, *M. D. Vice-President, and Professor of Natural History and Botany.*

Edward Miller, *M. D. Professor of the Practice of Medicine, and Lecturer on Clinical Medicine.*

Archibald Bruce, *M. D. Professor of Materia Medica and Mineralogy.*

Benjamin De Witt, *M. D. Professor of Chemistry.*

William J. M'Neven, *M. D. Professor of Obstetrics, and Lecturer on the Diseases of Women and Children.*

J. Augustine Smith, *Professor of Anatomy and Surgery.*

Published by order of the College.

NICHOLAS ROMAYNE, *M. D. President.*

Health of the City of New-York.

Some alarm and apprehension has been excited in New-York this season on account of the prevalence of a malignant fever at Brooklyn, a village on the Long-Island shore opposite the city. A number of cases have occurred there exhibiting strong appearances of yellow fever, and the disease has proved uncommonly mortal to those who were attacked by it. Several persons having resided in that village, or having had intercourse with it, have been taken ill in this city and died. The cases that have come under our own observation exhibited marked symptoms of uncommon malignity; but we know of no instance where the disease appears to have been communicated to any person within this city; nor have we heard of a single well authenticated case of malignant fever originating here. By the removal of many of the inhabitants from Brooklyn the disease there has been in great measure checked; and by the interdiction of the communication between that place and this city, all apprehension has ceased. The weather indeed has been so uncommonly cool and pleasant for some time past, that there is no reason to expect a visitation of the fever this year. Various opinions are entertained, as usual, amongst the Physicians of this city, with respect to the origin of the disease at Brooklyn; some ascribing it to indigenous sources, and others to importation. The great weight of professional opinion, however, is in favour of its local origin. We shall endeavour to collect the facts as they have occurred, and give them to the public in our next number.

New Edition of Sydenham's Works, and of Cleghorn's Minorca.

The medical public, we are confident, will be much gratified to learn that Dr. Rush is putting to press an edition of *Sydenham*, with Notes, and likewise of *Cleghorn*'s Diseases of Minorca. By this means these inestimable works will be accommodated to the climate and diseases of the United States. Dr. Rush will prefix to Cleghorn's Minorca, a short account of his life.

Death of Professor Woodhouse.

Died on Sunday afternoon, June 4th, 1809, in the 39th year of his age, Dr. JAMES WOODHOUSE, late Professor of Chemistry in the University of Pennsylvania.

Testimony of respect to the late Professor Woodhouse.

At a full meeting of the Medical Class of the University of Pennsylvania, held at Philadelphia June 7th, 1809, the following resolutions were unanimously adopted.

1st. Resolved, That in testimony of the high respect and affectionate attachment which we entertain for the late Professor Woodhouse, that each of us will wear crape on the arm for the space of one month.

2d. Resolved, That Professor Barton be requested to prepare an eulogium on the late Professor Woodhouse, to be pronounced before the Medical Class at their meeting next season.

Vacant Professorship of Chemistry filled.

At a meeting of the Trustees of the University of Pennsylvania, on Monday, July 10, 1809, JOHN REDMAN COXE, M. D. was appointed Professor of Chemistry in that University, in place of Dr. James Woodhouse, lately deceased.

Salt Spring.

A valuable salt spring has been discovered at Butler, in the State of Pennsylvania, in the bed of Connequenessing creek. On erecting a dam round it, it rose two feet above the level of the water in the creek in six hours. It is said to yield two and a half per cent. and to be apparently inexhaustible. Furnaces have been erected, and from the abundance of stone coal in its vicinity, the working of the spring has been found profitable.

Smith's Edition of Bell's Principles of Surgery.

This work, of which the prospectus will be found in our last number, is now in the press, and will be published in the course of the autumn.

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FOREIGN.

Accounts of additional Experiments concerning the Metallic Nature of the Alkalies and Earths.

From the London Monthly Magazine for May and June, 1809.

Having in our last given an account of Mr. Davy's discoveries with regard to potash, we shall proceed, as we proposed, to consider the properties and nature of the basis of soda. The basis or metallic substance obtained by decomposition, is a solid at the common temperature. It is white, opaque, and if examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is softer than any of the common metallic substances. It is a good conductor of electricity and heat, and small globules of it inflame by the voltaic electrical spark, and burn with bright explosions: its specific gravity is something more than 93. It becomes fluid at about 180° of Fahrenheit; but the exact degree of heat at which it becomes volatile, has not been ascertained.

The chemical phenomena, produced by the basis of soda, are in many respects analogous to those produced by the basis of potash: when exposed to the atmosphere, it immediately tarnishes, and by degrees becomes covered with a white crust, which deliquesces much more slowly than the substance that forms on the basis of potash, and which proves to be pure soda. The basis combines slowly with oxygen, and without luminous appearance, at all common temperatures: and when heated, this combination becomes more rapid; but no light is emitted, till it has acquired a temperature nearly that of ignition. In oxygen gas, it burns with a white light: in oxymuriatic acid gas, it burns vividly with a bright red light; saline matter is formed, which proves to be muriate of soda. When thrown upon water, it produces a violent effervescence, with a loud hissing noise; it combines with the oxygen of the water to form soda, which is dissolved, and its hydrogen is disengaged.

The basis of soda acts upon alcohol and ether in the same manner with the basis of potash. The water contained in them is decomposed; soda is rapidly formed, and hydrogen is disengaged. When thrown upon the strong acids, it acts upon them with great energy; if the nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acids there is much heat generated, but no light.

It combines with sulphur in close vessels, filled with the vapour of naphtha, with great vividness, with light, heat, and afterwards with explosion from the vaporization of a portion of sulphur, and the disengagement of sulphuretted hydrogen gas. The phosphuret has the appearance of lead, and forms phosphate of soda, by exposure to the air, or by combustion. The basis of soda, in the quantity of one fortieth part, renders mercury a fixed solid, of the colour of silver, and the combination is attended with a considerable degree of heat. It makes an alloy with tin, without changing its colour; and it acts upon lead and gold when heated.

From some very accurate experiments, Mr. Davy has found that 100 parts of potash consist of 86.1 of the basis, and 13.9 of oxygen: and in 100 parts of soda, there will be 80 parts of the basis, and 20 of oxygen.

To the question, whether the basis of potash and soda should be called metals, Mr. Davy says, that the greater number of philosophical persons answer in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination: their low specific gravity does not appear a sufficient reason for making them a new class; for among the metals themselves, there are remarkable differences in this respect; platina being nearly four times as heavy as tellurium; and in the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement; hence the basis of the alkalies are denominated Potassium and Sodaum.

In reference to his own discoveries, Mr. Davy observes, that, " In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of the alkalies, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic, and granitic rocks, there is a constant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutro-saline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them to simple elements."

From the fixed alkalies, the Professor proceeded to the earths, which are non-conductors of electricity. The alkalies become conducting substances by fusion: the infusible nature of the earths rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxygen would not admit of their bodies being acted upon by solution in water; and the only methods that proved successful, were those by which they were operated upon by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity, in metallic alloys, so as to obtain evidences of their nature and properties.

On this plan Mr. Davy undertook a series of experiments on Barytes, Strontites, and Lime, employing upon them the same methods as he had used in the decomposition of the fixed alkalies. Gas was, in each case, copiously evolved, which was inflammable; and the earths, where in contact with the negative metallic wires, became dark-coloured, and exhibited small points, having a metallic lustre, which, when exposed to air, gradually became white: they became white likewise when plunged under water; and when examined by a magnifier, a greenish powder seemed to separate from them.

He then made mixtures of dry potash in excess, and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit, as he had done in obtaining the metals of the alkalies. He hoped, by this means, that the potassium, and the metals of the earths, might be deoxygenated at the same time, and enter into combination in alloy. Metallic substances appeared less fusible than potassium, which burnt the instant after they had formed, and which, by burning, produced a mixture of potash, and the earth employed. He had found, that when a mixture of potash and the oxides of mercury, tin, or lead, was electrified in the voltaic circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium, was obtained. He tried the same on a mixture of two parts of barytes and one part of oxide of silver, very slightly moistened: when it was electrified by iron wires, an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point.

A mixture of barytes and red oxide of mercury, in the same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance that produced barytes by exposure to air, with the absorption of oxygen; and which occasioned the evolution of hydrogen from water, leaving pure mercury, and producing a solution of barytes. Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air and water.

While Mr. Davy was pursuing these experiments, he heard that Professor Berzelius, and Dr. Pontin, of Stockholm, had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them; and that in this way they had obtained amalgams of the metals of these earths. Mr. Davy repeated the experiments with a battery of 500, and obtained the most perfect success. The mercury gradually became less fluid, and after a few minutes, was covered with a white film of barytes; and when the amalgam was thrown into water, hydrogen was disengaged, the mercury remained free, and a solution of barytes was formed. The result with lime was precisely analogous, so also was that with strontites; with magnesia it was with more difficulty obtained. All these amalgams may be preserved a considerable period under naphtha; but in a length of time they become covered with a white crust. When exposed to air, a very few minutes only were required for the oxygenation of the bases of the earths.

In several cases, Mr. Davy exposed the amalgams of the metals of the earths, containing only a very small quantity of mercury, to the air, on a delicate balance; and he always found that, during the conversion of metal into earth, there was a considerable increase of weight. He also found that, when the metals of the earths were burned in a small quantity of air, they absorbed oxygen, gained weight, and were in a highly caustic or unslaked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids. Hence it is inferred, that the evidence for the composition of the alkaline earths is of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar; the inflammable matters in all cases, separating at the negative surface in the Voltaic circuit, and the oxygen at the positive surface. The professor denominates the metals obtained from the alkaline earths, *Barium*, *Strontium*, *Calcium*, and *Magnium*.

The professor next tried a number of experiments on the other earths, which are not alkaline; and from the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude that alumine, zircon, glucine, and silex, are, like the alkaline earths, metallic oxides. He admits, however, that the evidences of decomposition and composition are not of the same strict nature as those that belong to the fixed alkalies, and alkaline earths; for it is possible that in the experiments, in which silex, alumine, and zircon appeared to separate during the oxidation of potassium and sodaum, their bases might not actually have been in combination with them, but the earths themselves, in union with the metals of the alkalies, or in mere mechanical mixture.

The strong attraction of potassium, sodaum, and the metals of the alkaline earths for oxygen, led Mr. Davy to examine if their deoxydating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and he found that, when mercury, united to a small quantity of potassium, sodaum, barium, or calcium, was made to act upon moistened muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

The amalgam from ammonia, when formed at the temperature of 70° or 80°, is a soft solid, of the consistence of butter; at the freezing temperature it becomes firmer and a crystallized mass, and its specific gravity is below three. When exposed to air, it soon becomes covered with a white crust, which proves to be carbonate of ammonia.

"The more," says Mr. Davy, "the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear. Mercury, by combination with about the one twelve thousandth part of its weight of new matter, is rendered solid, yet it has the specific gravity diminished from 13.5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers, remaining unimpaired. It is scarcely possible to conceive, that a substance which forms with mercury so perfect an amalgam, should not be metallic in its own nature; hence it may be denominated ammonium."

From the preceding facts, the following questions have occurred: On what do the metallic properties of ammonium depend? Are hydrogen and nitrogen both metals in the aeriform state, at the usual temperature of the atmosphere, bodies of the same character as zinc and quicksilver would be in the heat of ignition? Or are these gases, in their common form, oxides, which become metallized by deoxydation? Or are they simple bodies, not metallic in their own nature, but capable of composing a metal in their deoxygenated, and an alkali in their oxygenated state?

Assuming the existence of hydrogen in the amalgam of ammonium, its presence in one metallic compound evidently leads to the suspicion of its combination in others. And in the electrical powers of the different species of matter, there are circumstances which extend the idea to combustible substances in general. Oxygen is the only body which can be supposed elementary, attracted by the positive surface in the electrical circuit; and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxygen. Hydrogen is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxygen; "may not then," says the Professor, "the different inflammable bodies, supposed to be simple, contain this as a common element?"

Should future experiments prove the truth of this hypothesis, still the alkalies, the earths, and the metallic oxides will belong to the same class of bodies. From platina to potassium, there is a regular order of gradation as to their physical and chemical properties, and this would probably extend to ammonium, could it be obtained in the fixed form. Platina and gold, in specific gravity, degree of oxydability, and other qualities, differ more from arsenic, iron, and tin, than these last do from barium and strontium. The phenomena of combustion of all oxidable metals, are precisely analogous. In the same manner as arsenic forms an acid, by burning in air, potassium forms an alkali, and calcium an earth; in a manner similar to that in which osmium forms a volatile and acrid substance by the absorption of oxygen, does the amalgam of ammonium produce the volatile alkali; and if we suppose that ammonia is metallized, by being combined with hydrogen, and freed from water, the same reasoning will apply to the other metals, with this difference, that the adherence of their phlogiston, or hydrogen, would be exactly in the inverse ratio of their attraction for oxygen. In platina, it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any of the metals, without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalies, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

Mr. Davy concludes by hoping, that the new facts which he has discovered may admit of many applications, and explain some phenomena in nature. "The metals of the earths," he says, "cannot exist at the surface of the globe; but it is very possible that they may form a part of the interior; and such an assumption would offer a theory for the phenomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat; for let it be granted that the metals of the earths and alkalies, in alloy with common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water must produce the effect of subterranean fire, and a product of earthy and stony matter analogous to lavas. The luminous appearance of those meteors, connected with the fall of stones, is one of the extraordinary circumstances of these wonderful phenomena. This effect may be accounted for, by supposing that the substances which fall come into our atmosphere in a metallic state; and that the earths, of which they principally consist, are results of combustion."

At the meeting of the Royal Society, February 2, a most curious and interesting paper, by Mr. Davy, was read, giving an account of various experiments on the action of potassium on ammonia; from which it appears that a considerable quantity of nitrogen can be made to disappear, and can be regenerated. When it disappears nothing can be obtained in its place but oxygen and hydrogen; and when it is formed, its elementary matter is furnished by water.

Messrs. Allen and Pepys have laid before the Royal Society an account of a great number of experiments, made with a view of ascertaining the changes produced in atmospheric air and oxygen gas by respiration; from which they infer,

1. That the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed; and therefore there is no reason to conjecture, that any water is formed by a union of oxygen and hydrogen in the lungs.

2. Atmospheric air once entering the lungs returns charged with from 8 to 8¼ per cent. carbonic acid gas, and when the contacts are repeated almost as frequently as possible, only 10 per cent. is emitted.

3. It appears, that a middle-sized man, aged thirty-eight years, and whose pulse is seventy on an average, gives off 302 cubical inches of carbonic acid gas from his lungs in eleven minutes; and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39,534 cubical inches, weighing 18,683 grains, the carbon in which is 5,363 grains, or rather more than eleven oz. troy: the oxygen consumed in the same time will be equal in volume to the carbonic acid gas. The quantity of carbonic acid gas, emitted in a given time, must depend much on the circumstances under which respiration is performed.

4. When respiration is attended with distressing circumstances, there is reason to conclude, that a portion of oxygen is absorbed: and as the oxygen decreases in quantity, perception gradually ceases, and we may suppose, that life would be completely extinguished on the total abstraction of oxygen.

5. A larger proportion of carbonic acid gas is formed by the human subject from oxygen than from atmospheric air.

6. An easy, natural inspiration is from 16 to 17 cubical inches, though this will differ in different subjects; and it is supposed, that the quantity of carbonic acid gas, given off in a perfectly natural respiration, ought to be reckoned at less than at a time when experiments are making on the human subject for the purpose, because in short inspirations the quantity of air, which has reached no farther than the fauces, trachea, &c. bears a much larger proportion to the whole mass required, than when the inspirations are deep.

7. No hydrogen, nor any other gas, appears to be evolved during the process of respiration.

8. The general average of the deficiency in the total amount of common air inspired, appears to be very small, amounting only to 6 parts in 1000.

9. The experiments upon oxygen gas prove, that the quantity of air remaining in the lungs and its appendages, is very considerable; and that without a reference to this circumstance, all experiments upon small quantities of gas are liable to inaccuracy.

Mr. Brande has laid before the Royal Society an account of the differences in the structure of calculi, which arise from their being formed in different parts of the urinary passages; and on the effects that are produced upon them by the internal use of solvent medicines. The experiments made by this gentleman were very numerous, and on an uncommonly large collection of calculi, to most of which histories of the case are annexed. The subject is divided into different sections: the first relates to calculi formed in the kidneys, and voided without having undergone any changes in the urinary passages. These are entirely soluble in a solution of pure potash: and when exposed to the action of the blow-pipe, they blacken and emit a strong odour, which arises from the animal matter which they contain, and which occasions the loss in the analysis of these calculi. Its relative quantity is liable to much variation. In one instance a calculus from the kidney, weighing seven grains, was ascertained to consist of

	Grains.
Uric acid	4.5
Animal matter . . .	2.5
	7.0

In some cases the calculi from the kidneys consist almost wholly of uric acid; sometimes phosphate of lime was combined with the acid.

I. In treating of the calculi which have been formed in the kidneys, and which frequently increase in that situation to a considerable size, he observes, that this augmentation is of two kinds:

1. When there is a great disposition to the formation of uric acid, the calculus consists wholly of that substance and animal

matter, so as frequently to form a complete cast of the pelvis of the kidney.

2. Where there is less disposition to form uric acid, the external laminæ are composed of the ammoniaco-magnesian phosphate, and phosphate of lime.

In one instance a small uric calculus was so deposited on the kidney, that its upper surface was exposed to a continual stream of urine, upon which beautiful crystals of the triple phosphate had been deposited. Mr. Brande therefore infers, that, under common circumstances, a stream of urine passing over a calculus of uric acid, has a tendency to deposit the phosphate upon it.

II. The calculi of the urinary bladder are of four kinds:

1. Those formed upon nuclei of uric acid, from the kidney.
2. Those formed upon nuclei of oxalate of lime from the kidney.
3. Those formed upon sand or animal mucus deposited in the bladder.
4. Those formed upon extraneous bodies introduced into the bladder. These are arranged under the following divisions:—First, Calculi, which from their external appearance, consist chiefly of uric acid, and which are chiefly or entirely soluble in a solution of pure potash. Secondly, Calculi composed chiefly of the ammoniaco-magnesian phosphate, or of phosphate of lime, or of mixtures of the two. These are characterized by their whiteness; by exhibiting small prismatic crystals upon their surface, and by their solubility in dilute muriatic acid. Thirdly, Calculi containing oxalate of lime, commonly called mulberry calculi. These are distinguished by the difficulty with which they are dissolved in acids, by their hardness, and by leaving pure lime, when exposed to the action of the blow-pipe.

By analysis a calculus of 60 grains yielded	Grains.
Urea and muriate of ammonia	5.2
Ammoniaco-magnesian phosphate . . .	6.
Uric acid	48.8
	60.0

From this, and many other experiments, Mr. Brande concludes, that the evolution of ammonia depends in all instances upon the decomposition of the ammoniacal salts contained in the calculus, more especially of the ammoniaco-magnesian phosphate, and that no substance which can be called urate of ammonia exists in calculi.

By analysis it was found, that a pure specimen of the mulberry calculus consists of

	Grains.
Oxalate of lime	65
Uric acid	16
Phosphate of lime	15
Loss in animal matter	4
	100

III. The calculi found in the urethra consist of ammoniaco-magnesian phosphate, and phosphate of lime, with a small portion of uric acid; though some appeared to consist almost wholly of ammoniaco-magnesian phosphate.

Mr. Brande, in the next section, has given the result of analysis of the calculi found in the horse, ox, sheep, rhinoceros, dog, hog, and rabbit. These were found mostly to consist of phosphate of lime and carbonate of lime in different proportions. In some, small proportions of animal matter were combined with the other substances.

The inferences drawn from these interesting and important facts are as follow:

That calculi formed in the kidneys, and immediately voided, are almost always composed of uric acid, and that the phosphates are very frequent ingredients in calculi of the bladder. They are uniformly deposited upon extraneous substances introduced into the bladder, but never form small kidney calculi. In what is commonly called a fit of the gravel, a small uric calculus is formed in the kidney, and passes along the ureter into the bladder. For some time after a stone has passed from the kidney, the urine is generally unusually loaded with uric acid, and deposits that substance upon the nucleus now in the bladder. After this, the subsequent additions to the calculus consist principally of the phosphates.

Where the disposition to form uric acid in the kidneys is very great and permanent, the calculus found in the bladder is principally composed of uric acid; but where this disposition is weak, the nucleus only is uric acid, and the bulk of the stone is composed of the phosphates. When the increased secretion of uric acid returns at intervals, the calculus is composed of alternate layers of uric acid and the phosphates. There are besides these many variations in the formation of the calculi.

In speaking of the solvents, Mr. Brande admits, that the internal exhibition of the alkalies often prevents the formation of the uric acid, and of course an increase of a calculus in the bladder, as far as the uric acid is concerned; but that its action will not proceed any farther; because, from his experiments he finds there is at all times a quantity of uncombined acid in the urine; and hence it follows, that, although the alkali may arrive at the kidneys in its pure state, it will there unite with the uncombined acid, and be rendered incapable of exerting any action upon the calculus in the bladder. Mr. B. also observes, that whenever the urine is deprived of a portion of the acid which is natural to it, the deposition of the triple phosphate and phosphate of lime more readily takes place, which is effected by the exhibition of the alkalies; and, therefore, though alkaline medicines often tend to diminish the quantity of uric acid, and thus prevent the addition of that substance in its pure state to a calculus in the bladder, they favour the deposition of the phosphates.

With regard to the exhibition of the acids, particularly the muriatic acid, in order to dissolve the phosphates, Mr. B. admits that, during the use of this acid, the phosphates are either diminished, or disappear altogether; and even the urine acquires sometimes an additional acidity, and therefore a solution of that part of the calculus which consists of the phosphates may be expected; but even then the nucleus of uric acid would remain, and thus a great deal of time would be lost without any permanent advantage. He is also decidedly against the injection of these solvents into the bladder at once, by means of instruments; because, in every case that has come under his observation, it has always aggravated the sufferings of the patient. He concludes, that as the nuclei of calculi originate in the kidneys, and that of these the greater number consist of uric acid, the good effects so frequently observed during the use of an alkali arise not from any actual solution of calculous matter, but from the power which it possesses of diminishing the secretion of uric acid, and thus preventing the enlargement of the calculus; so that, while of a very small form, it may be voided by the urethra.

National Institute.

Messrs. Gay Lussac and Thenard have given an account of the method which they adopted in decomposing the boracic acid. They put equal parts of potassium and pure vitreous boracic acid into a copper tube, to which a bent glass tube was fitted. The copper tube was placed in a small furnace, and the extremity of the glass tube plunged into a bason of quicksilver. As soon as the temperature was raised to 150° (Reaumur, we presume), the mixture became suddenly red, much heat was produced, the glass broken, and almost the whole of the air in the apparatus was driven out with great force. Only atmospheric air was disengaged, and a few bubbles of hydrogen. All the potassium disappeared, although it only decomposed a part of the acid. These substances were changed by their reciprocal action into an olive grey substance, which is a compound of potash and of the basis of boracic acid. The boracic radical was separated from it by washing it with hot or cold water. That which does not dissolve, is the radical itself, which possesses the following properties: this radical is greenish brown, fixed, and insoluble in water. It has no taste, nor any action on tincture of litmus, or on syrup of violets. Being mixed with oxymuriate of potash, or nitrate of potash, and projected into a red hot crucible, it entered into vivid combustion, of which the boracic acid was one of the products. The most curious and most important of all the phenomena produced by the boracic radical when placed in contact with other bodies, are those that it presents with oxygen. When four grains and a half of boracic radical were projected into a silver crucible covered with a jar, containing little more than a quart of oxygen, and the whole placed over quicksilver, a most rapid combustion took place, and the quicksilver rose to about the middle of the jar. The boracic radical exhibits the same phenomena with air as with oxygen, only that the combustion is less rapid. Hence it follows, that the boracic acid is composed of oxygen, and a combustible body: and that this substance is of a peculiar nature, and ought to be classed with phosphorus, carbon, and sulphur. It requires a great quantity of oxygen to change it into boracic acid, and it previously passes into the state of a black oxyd.

Operations for Carotid Aneurisms.

The operation for carotid aneurism has now been performed four times, twice by Mr. Cooper in Guy's hospital, once by Mr. Cline in St. Thomas's, and once by a Swedish surgeon. One of Mr. Cooper's cases terminated successfully, the patient now enjoying good health; the other was unfortunate, as was Mr. Cline's. We have not heard the event of the last. The unfortunate termination of the two cases was owing to circumstances unconnected with the operation.

POSTSCRIPT.

Report of the Brooklyn Committee of Health.

The undersigned Committee of Health experience the greatest satisfaction in being able to announce the entire disappearance of the fever which lately prevailed in this place.